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REPORT ON CEREBRO-SPINAL MENINGITIS.

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[CONCLUDED.]

From this summary, which is made up from the facts collected from various authentic sources, it will be perceived that the diseases just considered are *not* identical, but they really present wide differences, such as can leave no doubt of their *non-identity*. This parallel requires no discussion to evolve its teachings. While I have no doubt that cerebro-spinal meningitis may have imparted to it peculiar hues, so to speak, or tendencies, by the typhus, malarial, or erysipelatous "constitutions," or may be complicated with pneumonia or other special forms of disease, I feel I am warranted in concluding this to be a disease *sui generis*, and not a mere form of typhus, as it has been assumed to be.

If it has been shown that cerebro-spinal meningitis and typhus are not identical, it will hardly be worth while to show that the opinions above referred to, as to its identity with other forms of disease, have no sufficient foundation in fact.

In the third place, the evidence does not show that it is contagious. So far as the vast *majority* of cases are concerned, they are clearly not contagious in their origin. A large number of cases, however, are detailed by HIRSH, BOUDIN, RUM-

MEL, and others, which *seem* to show the disease is contagious. I will not here give the particular testimony on which its contagiousness is affirmed, but will remark on that evidence, that in diseases like the present, where comparatively only a few cases have occurred which favor the idea of contagion, we may well scrutinize them closely, before we admit them.

In diseases like small-pox, *e.g.*, when, at least in the majority of cases, we can show exposure to contagious influence clearly to have *preceded* the occurrence of the disease, and in which we can clearly communicate the disease by contact or inoculation, and in which it presents a series of distinct stages and is self-limited, I say in *such* cases, there can be no doubt. But it is not so in this disease. If we *admit* contagion, it is clear the majority of cases do not occur from it, but under circumstances forbidding such a conclusion. Indeed, there is, perhaps, not a single law of contagion to which it conforms only in seeming, while it *does* conform to the general facts or laws of epidemic influence.

In determining the action of contagion in such a case as this, it can only be done on the observed order of sequence. To be valid, it should be shown in the majority of cases. But here, on the contrary, comparatively only a few have been observed. In the majority of cases, no such order has been observed or has existed. Besides, in those cases in which it *has* seemed to operate it has not been shown that such cases were not among people who were living under the operation of the same cause or causes, which, producing disease in *one*, really (as seems most probable) produced the same disease in due time in others. The evidence which has been offered in support of the belief in contagion, can only yield that support by a palpable *petitio principii*. On the whole, the evidence seems to me not to support the question of contagion.

Is the disease infectious? Most authors decide, or seem inclined to decide, it is. There has always seemed to me more than the usual looseness with which medical terms are employed, in the use of the terms "contagion" and "infection." I will state what I understand these terms to mean:—

By *contagion*, I understand such a positive cause of disease as, emanating from a sick person, may be communicated *through the air*, to *another* person, so as to produce in such person the same form of disease. As a good example, I would mention small-pox.

By *infection*, I mean a positive cause of disease, such as may be communicated from a person diseased, by *contact*, to another person not diseased, and which is capable when thus communicated, as in the case of contagion, of producing or reproducing the same form of disease which was the occasion of it. As examples, I would mention syphilis, hydrophobia, etc., diseases which are *not known* to be communicated through the air or by contagion, but *are known* to be communicated by contact.

But some diseases are both contagious and infectious, as small-pox, erysipelas, and, *probably*, typhus. I would call those diseases contagious only which are clearly capable of affecting persons through the medium of the air, in the way already mentioned. Those infectious only which are capable of being propagated in the way mentioned above. Such diseases as have their *causes* communicated by contact, and in this way are propagated, as in scabies or porrigo, rather than the *products* of diseased action, as in small-pox, hydrophobia, syphilis, etc., *might* or *might not* be called infectious, according to one's pleasure. If this be a correct definition of infection, I must say there is no evidence worthy of note that the disease is infectious.

Having, in some measure, seen what the disease is *not*, let us now turn to the direct and important question:—What does the evidence teach us the disease *is*? What is its *seat*, its *nature*? These questions will be found difficult as they are important.

Having concluded the essential cause is external, it now becomes us, in view of its nature, to trace its effects as observed in the blood and textures of the body, and, as near as possible, the *order* in which they appear, and, if *possible*, *how* the phenomena are produced. It will be admitted that the cause, whatsoever it is, must first enter the blood. Here, the blood may serve simply the purpose of a vehicle, conveying the poi-

son to the parts on which it acts; or the blood may form the *nidus* for the development of the poison in quantity sufficient to produce its visible effects, as in small-pox or hydrophobia, probably; or the poison (if we may so call it for the time) may initiate a series of changes in the blood which, in their turn, become the cause of the observed phenomena. Some of the facts already given will be called up in the statements which follow, but for a different purpose than before. This statement will, I hope, save me from the charge of thoughtless repetition.

That the poison, whatsoever it may be, *sometimes* acts directly on the nervous system, seems to be matter of fact. It must be distinctly remembered that the question is not whether there *are* changes in the blood, but whether those changes become the cause or causes of the symptoms and appearances presented in this disease.

Now, though we are not able to say positively, *no* blood-changes have occurred sufficient to account for the disease, its sudden attack, or speedy fatal results, yet, on the other hand, our *experience* in regard to blood-changes in other diseased states is opposed to a belief that death is due to changes in the blood, rather than to a poison suddenly introduced into, or developed in the blood, though independent of it. Our *experience* (and this is all we can rely on here) makes it easier for us to believe the latter than the former. There are many obvious instances where known poisons have been introduced into the blood and have caused death in as short a time, in a way not very dissimilar, and which have left behind no greater traces of their action than some of the reported cases have of sudden death in this disease. It is difficult, with our present experience, to imagine blood-changes so grave take place in a time so *short* as the suddenness of the attack requires they often should. I find it difficult, if not impossible, to resist the conviction that some energetic poison *has been* introduced into the blood and has acted directly on the nervous system, sufficiently concentrated or violent, in some cases, to destroy life speedily, by destroying the energy or power of the organic nervous system, striking thus at the very fountain of life; or, without acting in this



marked manner, initiating a series of changes in the blood and tissues, which, in process of time, and according to circumstances, may result in death or recovery. I am reminded very much of the action of hydrocyanic acid, strychnia, morphia, etc., when I witness or consider such cases.

As already remarked, we do not *know* there have been no changes in the blood sufficient to account for the phenomena of this disease, but what we *do* know does not render such probable. But whatsoever uncertainty there may be about this, the evidence goes to show the nervous system as earliest and most profoundly affected, both sympathetic and cerebro-spinal, especially the former, and the symptoms are, in great measure, confirmed by the *post mortem* appearances.

The sudden and invariable reduction of temperature, and the change in the character of the pulse, always indicative of a loss of energy or power, and perhaps other symptoms, point at once to the organic nervous system as the part primarily affected, so far as the *earliest symptoms show*. We know enough of the power and influence of this nervous system in the organic processes or actions of the body, and especially those comprehended under the title of nutrition, not to be surprised that any grave affection *it* should experience would be attested, or followed in due season, by such marked phenomena as even *this* disease presents.

After a careful consideration of the phenomena, the conclusion arrived at seems to me most in accordance with them, *viz.*: The organic nervous system (and, probably, at the same time the cerebro-spinal) is acted on in various degrees by some subtle epidemic cause, unknown save in its effects, which directly impairs its energy, and, through it, probably initiates those changes in the blood and textures to which many of the symptoms during life and appearances after death are due.

Whether the blood is primarily affected, other changes following as consequences, is true or not, we have seen that changes of a marked character do occur, and that they are, probably, not due to retained excretory matter, or to a deprivation of some of its appropriate elements, we have already seen. During life, the blood is characterized by unusually

bright color, by firm coagulation, by large clot, often by a buffy coat, and by an increase of fibrine and corpuscles; after death, by fluidity and dark color, though not always the former.

As regards the fibrine, in whatsoever way we account for its increase, or whatsoever effects we may attribute to it, it seems probable it is owing to its excess we have the remarkable tendency to plastic exudation on the surface of the brain and spinal cord, where time has been afforded for it to occur. One reason *why* this effusion takes place so freely on this surface seems to be, that it is so surpassingly rich in vessels, on the one hand, and that they are *so superficial*, on the other hand. Whether the fibrine plays any other important part than this, seems mainly matter of conjecture.

As regards the red corpuscles, whether it is negative, as by a failure to disintegrate, or positive, as by reason of increased production, and as to what share they may have in producing the phenomena of the disease, in relation to all such points we have no very definite information.

Albumen, as we have seen, has been found in the urine in many cases, especially in the later stages or during convalescence. But the absence of dropsies, except to a limited extent, and of all the symptoms usually connected with the blood impoverished of its albumen, would lead one not to lay much stress on *this* feature. Whether the salts are less in quantity than normal in the blood the facts do not show.

It will be remarked of these blood-changes, so far as they have been matter of observation, they have been witnessed only after several days of the disease have passed.

That in cases which have had some duration, the blood after death is darker and more fluid than it is customary to find it, may be significant. But these same conditions, so far as we can tell, exist, on the average, in a more perfect degree in forms of disease widely different from this, and, besides, as we have seen, they do not obtain in all cases. It must be remembered, the blood during life does not show anything of such a state, but the contrary. It is no uncommon thing, in fact, to find a similar state of the blood in cases where death has occurred from violent diseases.

The view I should be inclined to take of the blood in this disease, is, that whatsoever changes appear in it constitute simply a part, and a very important part, of the disease, and that these changes are probably subsequent, in order of time, to the affection of the organic nervous system. The diseased states of the blood may, and probably *do*, have an important causative relation to some of the later phenomena of the disease.

If we take all the leading phenomena of the disease, from beginning to end, and regard simply the direction they point, it will be readily seen they point almost unanimously to the nervous system. The chilliness, the state of the pulse, the sympathetic vomiting, the mental disturbance, the disturbance of general and special sensibility and of motion, all such symptoms point to the nervous system as the *characteristic seat* of the disease.

Having thus reviewed the symptoms and blood appearances, and having, in some degree, ascertained the truths to which they seem to point, let us now turn to the pathological appearances and interrogate *them*, and find if their teachings correspond to those the symptoms have apparently afforded.

When we consider the *post mortem* appearances, we find, excepting those found within the cerebro-spinal cavity, none sufficiently constant to be considered as properly belonging to the disease. They are simply incidental, or throw no light on the disease not already given in those observed in the cerebro-spinal cavity. This being admitted, and the blood appearances having already been considered, our attention will be confined to the central nervous system and its envelopes.

As regards the contents of this cavity, they have never been found wholly free from at least some evidence of disease. The most constant, the uniform morbid appearance, has been congestion.

As regards exudations, they always seem to be present, where time has been given for them to appear in noticeable quantity. But before we consider the character of the effusion, let us see clearly where the *post mortem* appearances occur.

The *dura mater* is probably always either congested or has

abnormal fulness of its sinuses. It is almost never structurally diseased or exhibits in a noticeable degree a plastic effusion.

The *arachnoid* is also very seldom diseased, especially on its outer surface. It is many times somewhat discolored, roughened, and thickened as we should expect a delicate membrane like this to be, bathed on one surface in exuded matter. It is *very seldom* structurally diseased. Those who will remember how sparsely this membrane is supplied with vessels and nerves will not be much surprised at this. The contents of its cavity seldom present marked evidences of disease.

But when we turn to the *pia mater*, the case is changed. It is *never* free from some evidence of disease, and in almost all cases evidence the most indubitable, such as marked effusion, plastic and otherwise; if not *this*, congestion. The effusion, in such places and to such extent as has been already indicated in the *post mortem* history. Now, this effusion is almost uniformly spoken of as occurring on the serous surfaces of the body, for which the disease is believed to have a decided preference, especially the serous surface within the cerebro-spinal cavity. But it so happens, as regards the latter, that it almost never is the seat of the disease. The *only* part constantly diseased is the *pia mater*, which is not a *serous* membrane any more than the periosteum is.

It will be remembered that this membrane is simply a fibrous investment for the brain and spinal cord, being thickest on the latter. It forms not only an investment for the brain and cord, but a trellis or framework, so to speak, in the meshes of which the arteries destined to enter the cerebral substance ramify and subdivide to the requisite degree of fineness. It is *fibro-vascular*, and *not* serous. Its extreme richness in bloodvessels, and their *superficial character*, will probably, in some measure, explain why this membrane is so often the seat of disease, and why the effusion occurs on it in such abundance. Something may be due to the fact, that the cerebral circulation occurs in an unyielding airtight case, in this respect being different from the circulation elsewhere, save in the bones, perhaps. I have long thought the conclusions reached by Dr. BURROWS, regard-

ing the cerebral circulation, were not altogether sound, and that the fact should be held as important that the cerebral circulation takes place under the conditions it does. In this connexion, I merely suggest this as a matter to be thought of in explaining, or seeking to explain, the phenomena of the disease now under consideration.

Another fact is worthy of note, that the effusion is most abundant on those parts most vascular, as on the under surface of the brain, where the pia-mater covers the under surface of the great ganglia which are richly supplied with vessels.

But why the *cause* exerts its influence *here* so constantly, can only be explained when we are able to explain why opium acts on the brain, strychnia on the spinal cord, one medicine on this part, and another on that. Whatsoever peculiar effects such medicines produce, they stand to such effects in the relation of cause, it is almost superfluous to say. In this disease we have certain *effects*; whatsoever the essential cause may be, they are, in the main, *its* peculiar effects. It is no more difficult for us to see how the supposed cause of this disease shall produce its characteristic effects, than for us to explain to ourselves why the medicines named should act as they do, rather than some other way or place.

It now becomes a question, whether this supposed cause acts on the brain and spinal cord directly or indirectly. I am prepared to admit its direct action on the brain and spinal cord. But whether this be true or not, I deem it possible to show, as highly probable, that the results which appear within the cerebro-spinal cavity are due, in no small degree, to the state of the organic nervous system. I will shortly return to this topic.

In cases where effusion has occurred to any considerable extent it is not difficult to account for the coma and many of the other symptoms, more especially when the brain and spinal cord are substantially diseased, as they often are. The derangements of the nutrition and of structure, the pressure, and the like would be considered sufficient causes for death without the direct effect of the supposed poison.

As regards the character of the effusion, generally speaking,

it resembles what we find in other congestions and inflammations so closely, that no difference except in degree is to be perceived. The exuded matter has in many epidemics appeared purulent in a marked degree, as, indeed, we would occasionally expect it to be in any epidemic. Though this purulent character is more common in this disease than other forms of inflammation of the same parts, yet it is by no means *peculiar* to this disease. The most that can be said is, that the disease shows a more decided tendency to the exudation or formation of pus than ordinary. This will be an interesting point to bear in mind in the therapeutical management of the disease.

There are several points which have been established, with various degrees of certainty, by recent anatomical and physiological investigations, which, if held in view, will assist us in understanding or interpreting the phenomena of diseases affecting the nervous system. Among them may be mentioned, that it is pretty well established that the cerebrum or the gray matter on its surface is the seat of those automatic actions such as we become *conscious of willing*, called *volitions*, and also that part of the brain immediately concerned in intellection, and to be the part wherein we become conscious of sensory impressions. All sensations, as such, are recognized here; all volitions, as such, originate here. Beneath this great primary centre, and subordinate to, and partly imbedded in it, are certain other centres, such as the corpora striata, which are probably the centres to which all *voluntary* impulses converge from the surface of the hemispheres, previous to their distribution to the voluntary muscular system; the thalami optici, to which converge, probably, all *general sensory* impressions, previous to being transmitted to the cerebral hemispheres for recognition; and the cerebellum, which has, probably, as its chief function, the coördination of the voluntary muscular movements. About these *latter*, are massed the various centres for the special senses. I say, by bearing such facts in mind, we will not be surprised to find derangements, in various degrees, of the functions those parts seem destined to perform, when we find, as we do in *post mortem* examinations, they are so frequently dis-

eased, or find evidence of disease in their immediate vicinity. By remembering such facts, we are many times able to connect *post mortem* appearances with the symptoms of the disease, or, conversely, to determine, with more or less certainty, from the symptoms, the situation and extent of the pathological changes going on during life, a very desirable thing to accomplish when possible.

But, returning from this digression, let us now recur to the question, as to what share the state of the organic nervous system has in producing the diseased or abnormal condition of the circulation, and the consequences which flow from it, as regards the cerebro-spinal axis.

This cannot well be shown, without a reference to certain facts and experiments, and to certain views which I think the facts warrant, which, though it may be a digression, they are, however, of sufficient importance to justify their introduction in this place. Neither the facts nor the views can be said to be new, but their importance has never been sufficiently recognized, so far as I know.

\* At one end of the elastic arterial tube is a powerful muscular apparatus, the heart, by which this elastic tube is kept constantly distended, and which communicates, in that class of animals possessed of it, the original impulse to the blood. Part of the force exerted by the heart is expended in setting the blood in motion; part, in opposing the elasticity of the arteries, by which latter the force so expended is *conserved*, and in the interval between one ventricular contraction and the next which succeeds it, is expended mainly in reacting on the contained column of blood. In obedience to this force, the blood being prevented from returning toward the heart, is forced onward steadily toward the distal end of the artery. By the time the blood has reached the distal end of the artery, its momentum has become so much diminished by friction and the like that it could probably not be circulated through the numberless small, winding capillary passages, were it not for some

\* These same views are more fully presented in a paper to be found in the August No. of the *Chicago Medical Examiner*.



arrangement, either at the distal end of the arteries or in the capillaries, for communicating a new impulse to the blood which has just entered, or is about to enter, the capillaries. But the capillaries have been shown not to be muscular. They have very thin and delicate walls, just sufficient, apparently, to separate the blood within from the tissue without the vessel. If *anywhere*, the muscular structure for communicating this new impulse to the blood must be nearer the heart. Now, it has been demonstrated that the small arteries or arterioles have muscular walls. Manifestly, this is not the place to refer to all the experiments which have been or may be made to prove this fact; but I will refer to one or two:—

The brothers WEBER found, upon drawing out the mesentery of a frog and placing it under the microscope, if they transmitted through one of the small arteries a galvanic current, it immediately contracted, and by continuing the current, the artery would diminish in size *very much*; but if the current was too *powerful*, or too *long-continued*, the artery ceased to contract, and continued to dilate until much greater than the natural size—the muscular tissue, in the last case, losing its power to contract on the application of appropriate stimuli. The same phenomena may be observed, even more strikingly, in the wing of the bat, with the additional advantage that it is a warm-blooded animal.

We will, then, admit the muscularity of the small arteries, and that they readily contract on the application of stimuli, as other muscular organs do, and that this muscular tissue may be paralyzed from excessive stimulation, or from the want of it.

Another question now arises, whether these small arteries, like other muscular organs, from which they differ essentially only in size, are supplied by nerves. They are, as has been and can be demonstrated, and especially do these small arteries receive twigs from the organic nervous system.

Now, are there any circumstances known, by which we can know the muscular coat of these vessels to act under a stimulus from this nervous system. If we reason from analogy, they do. When we take a swallow of water or a morsel of food into the

esophagus, for example, it immediately excites, by reflex action through some of the neighboring ganglia of the sympathetic system, contraction of the esophagus, by which the fluid is propelled along in a certain direction.

The only difference worthy of notice in the two cases seems to be that of size. You have in both cases a muscular tube supplied by twigs from the organic nervous system and a fluid, filling it rhythmically, or with an impulse. How can we refuse to admit contraction in the one case, when it is plain it occurs in the *other* case? If this muscular apparatus is not here for some such purpose as this, what purpose does it fulfil? We will admit it is here for the purpose of contraction, and that it does contract rhythmically, that it does so for a purpose, and that the purpose is that the blood may be propelled into, or even through, the capillaries which have no such coat, and, therefore, no such power. I will now mention one experiment, which seems to show these vessels are really under the control of the organic nervous system:—

If the sympathetic nerve be divided on one side, in the neck of a rabbit, immediately afterwards the small arteries in the conjunctiva and in the ear are seen to become dilated, congestion, in fact, results, just as we would suppose it would, by removing from the vessels the regular or rhythmical supply of motor stimulus, by dividing the nerve leading to them, by which the exciting power transmitted, on which the contraction of the vessels depends. We will then admit the small arteries contract rhythmically under an influence afforded by the organic nervous system. If this is true, it follows if this nerve stimulus, for any reason, should be supplied in great excess, in the same proportion would these small arteries contract beyond what is normal; or if, on the contrary, we suppose this stimulus for any reason is withdrawn, either partially or wholly, from the vessels of any part, the effect would be their paralysis with consequent relaxation and distension or *congestion*. In so far, then, as the assistance of these vessels is necessary in the circulation of the blood, or so far as they have been diminished in activity, just so far will the circulation be retarded. *This is*

congestion, the indispensable preliminary step to the *post mortem* changes which follow in due order, as I will undertake to show in another paragraph. This may all, in the analysis, be traced to some cause which shall so act on the organic nervous system, or some part of it, as to destroy partially, or entirely, its influence over the vessels of some part or parts in the way already referred to. The agent, whatsoever it may be, by which this state is brought about, is what I suppose to be the *essential cause* of the disease. That there are known agencies which act not in the same, but in a *similar* way, I deem it unnecessary to show; and that this cause should act particularly on that part of the nervous system which controls the cerebro-spinal circulation, seems to me not any more strange than the peculiar action of many medicines which act on special parts of the body. If we reject the one because we cannot afford a rational explanation, for the same reason we must reject the other, but which, as a fact, no one thinks of disputing. We will then suppose the essential cause of the disease so to act on the organic nervous system, or that part, more *particularly*, which controls the circulation of the cerebro-spinal axis, as in various degrees to destroy its influence over the same, namely, the small arteries which ramify *so richly* in the *pia mater* and ventricular walls of the brain, and which do not have, as the larger arteries do, an *elastic* but a *muscular coat*. I should expect, *a priori*, any consequence which would flow from this state of the vessels to be most plainly marked where the vessels are most abundant, or on those parts of the *pia mater* most richly supplied with vessels, namely, parts at the base of the brain, along its fissures, sides, and upper surface of the brain, and on the medulla oblongata, and cervical and lumbar portions of the cord. This accords *entirely* with the *facts* of pathological anatomy.

Now, I suppose this poison to be capable, sometimes, of acting so energetically in destroying the excitant energy of the organic nervous system, and probably directly on the brain and spinal cord, as to produce death so soon that no considerable traces of disease shall be left to be observed after death, suffi-

cient to account for the fatal result. Again, I suppose it may so act as to produce disorder of the capillary circulation, especially that of the cerebro-spinal axis, in the way already indicated. In various degrees its controlling influence over the circulation is destroyed. There is paralysis, more or less complete, of the muscular coat of the small arteries, the chief function of which, aside from that of a mere conduit, I believe to be to communicate a new impulse to the blood, *necessary* to circulate it through the capillaries. What I suppose to take place in the pia mater, *e.g.*, is *similar* to what is *observed* to take place in the conjunctiva or ear of a rabbit when the influence of the organic nervous system is withdrawn from the muscular vessels. I suppose the same phenomena to occur here as in the mesentery of a frog or the wing of a bat, at or toward the close of the experiments detailed. The vessels cease to contract, they relax, increase in size, not only for this reason permitting the blood to accumulate, but because they have lost the power to propel the blood onward, which is necessary to circulate it through the capillaries, which latter are entirely passive, as regards the circulation of the blood.

This is what I understand as *congestion*, and it results *not* from more blood being *sent* to the part, but because of increased size or capacity of the vessels, and because it is *detained* there through failure of the propulsive power of the small arteries. Blood is *constantly sent* to the part, but imperfectly sent away. Two conditions now favor transudation:—1st. *Stretching* or *thinning* of the walls of the relaxed vessels. 2d. *Increased expansive pressure from within outward*, because of a constant increase in the *volume* of the blood. I suppose the serous or watery part of the blood to exude *first*. Such is also the teaching of morbid anatomy. In it, and more especially a little later, the more solid elements of the blood, as the *fibrine* and *albumen*, or even the *corpuscles* at last. The blood thus detained in the vessels and made *viscid* by *loss of fluid*, I suppose to *coagulate*, and to be made here and there *utterly incapable* of being circulated, in this way permanently *obstructing* the vessels. The trouble once fairly initiated, it is easy to see how the

effusion may be increased, how nutrition of the brain and cord will be interrupted, and how, through this and the *pressure* caused by the effusion, etc., we may have produced many of the symptoms of this disease or even death. In this same way, would I account for most effusions or exudations *anywhere*, either in the tissues or on free surfaces, whether in this disease or others. This distension of the small vessels I suppose may go on, determined by many circumstances, in some parts, until the vessels shall give way and true hemorrhages occur here and there, especially in the tissues, and thus the ecchymosed spots seem many times produced.

The changes which the effused matter will undergo, will depend, of course, on a variety of circumstances, which it is not our purpose to enquire into here. I do not deem it necessary to suppose the muscular coat of the small arteries, in *this* case, to have lost the *power* to contract, but simply that the excitant they normally should receive from the organic nervous system to have failed.

It seems to be the province of the cause of this disease to produce a state of depression or sedation of the nervous system, especially the organic. While there are many known agencies capable of producing sedative effects on this nervous system, the peculiar character of this cause seems to be that of acting in a marked manner on that part of the sympathetic system which controls the circulation of the cerebro-spinal axis.

At this time and in this paper, it would be manifestly improper to go farther in adducing proof that this explanation is a reasonable, perhaps a true one. I should not have entered on the preceding statements, if they had not seemed absolutely necessary to enable me to explain what I have learned to regard as the pathology of the disease. I would gladly have referred to some work in which the views glanced at in relation to the circulation are, especially in the small vessels and the relations sustained thereto by the organic nervous system, given, but I have not seen a satisfactory statement of such views in any author. I have examined these questions with some care, as shown elsewhere. They are introduced incidentally only. In

a paper just referred to, I have examined, briefly, the principles glanced at in this discussion, and have shown their applicability in the explanation of many pathological states, as well as in the present case.

PATHOLOGICAL DEDUCTIONS, ETC.

In that part of the discussion over which we have passed, we seem to have reached the following conclusions:—

1. The symptoms which refer to the nervous system, while they vary, are, considered as a group, never absent. Other symptoms are mainly incidental.

2. The pathological appearances found in the cerebro-spinal cavity are, in some degree, always present, generally in a marked degree. Other pathological appearances mainly incidental. Contrary to the general opinion, it seldom attacks the serous surfaces within the cerebro-spinal cavity or elsewhere.

3. The blood before death uniformly shows a bright crimson color, coagulates rapidly and firmly, and often exhibits a decided buffy coat. Upon examination, it shows a decided increase of fibrine and, generally, of corpuscles. After death, the blood is generally, but by no means always, darker and more fluid than customary.

4. It is *not* a form of typhus.

5. It is probably neither contagious nor infectious.

6. It is probably due to a special external epidemic cause.

7. This supposed cause probably acts on the nervous system directly, perhaps primarily, whatsoever its action on the blood, so powerfully sometimes as to destroy life immediately, at others so as to induce, through the organic nervous system, disorder in the *circulation* and organic processes *dependent* on this nervous system, of the former, in a constant, special, and marked manner in the brain and spinal cord, entailing a series of consequences, where time is given, in this way, and probably by direct action of the cause on the blood, sufficient at last to produce death, independent of the real cause of the disease.

That the nervous system may recover its energy after the first impression of the poison, but through certain consequences, as effusion into the cerebro-spinal cavity, the disease may be

continued for weeks or even months, either to death or a slow and painful recovery.

8. That the blood is probably not primarily diseased but becomes notably changed in some respects *during* the *progress* of the disease, and, doubtless, contributes much after a time to the unfavorable or fatal results in cases which have some duration. That the state of the blood is, in some measure, possibly due to the direct action of the poison, but seems to be mainly due to the disorder of circulation and nutrition, induced by the above mentioned state and influence of the organic nervous system.

9. The disease may take a distinctive hue or character, so to speak, from the prevailing forms of disease, as erysipelas, malarial or periodical diseases, typhus, etc., and that it may be complicated with various *special* forms of disease.

10. That it is a *cerebro-spinal meningitis*, if we name the disease from the standpoint of *a group of its most obvious and constant phenomena or effects*, and all other titles given from similar standpoints, as "spotted fever," deserve to be abandoned for obvious reasons. That it is impossible, at this time, to give the disease a name from the standpoint of *causation* which shall be satisfactory.

#### THERAPEUTICAL DEDUCTIONS.

We seem to have two classes of cases in which the indications for treatment are somewhat different. Other subdivisions, more or less useful, have been or might be made, but the two classes here given seem to comprehend most, if not all, cases. They are:—

(a) 1. Those in which the cause operates with such intensity as to destroy life directly, before time has been given for pathological changes of any considerable importance to occur.

2. Those in which death does not occur directly from the effects of the poison, but in which the nervous system recovers from the first impression, and in which, time being given, pathological changes occur, as within the cerebro-spinal cavity or in the blood, many times sufficiently marked to become the cause of death or a protracted recovery, independent of the agency



of the primary cause, and which cases at last show much of the character of ordinary inflammation of the contents of the cerebro-spinal cavity, are often sthenic or febrile in character, and require a quite different therapeutical management, in many respects, from those of the first class.

(b) If the views we have adopted be correct, then, upon well-recognized principles in therapeutics, we should be led, *a priori*, to a course of treatment in general as follows. In view of the first class of cases, and the actual phenomena of the disease, as they appear at an early period, we would turn:—

1st. To such known agencies as would efficiently stimulate the organic nervous system, such as opium, cantharides, camphor, chloroform, strychnia, the active principles of coffee and tea, quinine, brandy, etc.

2d. To such external agencies as would prove revulsive, by promoting the circulation at the surface of the body, and which would at the same time prove indirectly stimulant to the nervous system, such as blisters, heat, frictions, stimulating embrocations, and sometimes the alternation of heat and cold, dry cups, local bleeding occasionally, faradisation of the skin, etc.

3d. We would not, *a priori*, approve of general blood-letting or any general sedative measures at this stage, only in exceptional cases, nor would we in this stage depend much on alteratives, such, *e.g.*, as the mercurials.

4th. In the second class of cases, we would expect internal stimulants not to answer so well as in the first class of cases, and in the early stages of the disease. External stimulation, as blistering, we would expect would prove useful over the back of the head, neck, and spine. We should be led to think more of the application of cold, of blood-letting, both general and local, and of alteratives, such as the mercurials, iodide potassium, bromide potassium, etc., than in the former cases.

Such, in brief, are some of the therapeutical deductions, from the views we have arrived at, concerning the nature of the disease.

#### THERAPEUTICAL HISTORY.

It now remains for us to consider the *therapeutical his-*

tory of the disease, and in so doing, not only to determine what the treatment actually *has* been, but what has been the most *successful* treatment, and how far it agrees with that which, in the light of general principles, we have deduced from pathological considerations, and still farther try and see what light from its therapeutical, can be thrown back on its pathological history. Keeping in view the two great classes of cases already referred to, I will consider the principal remedial agencies which have been employed in this disease.

I. SEDATIVES.—In a general way, they have not been found useful in the early part, but have found more favor in the latter part of the disease, and in cases distinctly febrile.

1. *Blood-letting*.—Mons. MATTOT, of Auch, BASSERON, of Alger, CORBIN, of Orleans, TOURDES, of Strasbourg, MAILLOT, of Lisle, and others, have bled very freely from the arm, and by cups and leeches; but lost more than 60 per cent of their cases. They generally bled early, and the bleedings were often repeated in some cases, but with unsatisfactory results. Dr. DRAPER, however, and those to whom he refers, seem to have derived advantage from local bleeding. Drs. MINER and TULLY, and Dr. KEMPF, are decidedly opposed to blood-letting on practical grounds. The same conclusion has been reached by HIRSCH and RUMMEL, in the Dantzic epidemic. They repudiate general, but in some cases have resorted to local bleeding, apparently with good effects. Much the same is the testimony of NIEMEYER. BROUSSAIS would, however, bleed in the beginning from the arm, in most cases, and resort subsequently to local bleeding in the most active cases. FORGET, LEVY, and some others, would bleed locally, and in some of the most sthenic cases would resort to general bleeding. And so, in general, has it been with the majority of observers whose experience has been sufficiently broad to be useful in guiding us to correct conclusions. The weight of experience is against its employment, except with great caution, and principally in the latter part of the disease. Local bleeding, by cups and leeches, has most favor.

2. *Cold*.—Cold is here considered only in its primary effect.

While some have found cold to the head and spine not useful, the majority of observers, perhaps, have had apparently satisfactory results. Its application is attended with most benefit after reaction, and in cases decidedly febrile in character, while early in the disease, and in those cases marked by extreme depression, it has not seemed to be attended by happy results. Among those who have found it useful, may be mentioned RUMMEL, who has found it good, almost from the beginning, in cases where the face was flushed, the head hot, and where there was much pain; but in cases where the face was pale, not much pain, stupor, and the pulse low, he has not seen it followed by good effects, but the contrary. HIRSH and NIEMEYER report much the same. PANTHER liked the effect of a cold shower after a hot bath. FOVILLE, BROUSSAIS, ROLLET, and others, both in this country and Europe, concur, in the main, in the statements just made. The employment of cold, seems to have been most favorable early in the disease, when opium has been administered internally.

3. What has been said of blood-letting may be said of other sedative agencies, such as veratrum, aconite, belladonna, stramonium, and other remedies which have a similar action with them. But few or none of them have had an extensive trial, but so far as they have been tried, they have not yielded encouraging results. Belladonna has, perhaps, the most evidence in its favor. In active or sthenic cases, such remedies have generally proved useful, if I rightly collect the scattered testimony concerning their use. Digitalis, if it can in all cases be considered sedative, has seemed to give some good results. RUMMEL speaks highly of its use in the first few days of the disease. ROLLET speaks favorably of laurel water, and MIAHLE of hydrocyanic acid. Such, in general, seem to be the results of experience in the use of sedative agencies in this disease.

II. STIMULANTS.—They have been found useful at all periods of the disease, especially in proportion as they are early employed, both external and internal.

(a) *Internal.* 1. *Opium.*—This stands not only at the head of the list of stimulants, but also at the head of remedial agen-

cies in this disease. There is none other, concerning which there is such unanimous and emphatic testimony. It has been employed in all stages, and in all kinds of cases, but has, generally speaking, proved most efficient in the early part of the disease, and in those cases which display the least acuteness in character. RUMMEL found it most efficient where the face was pale, pupils dilated, pulse low, etc., and in acute cases, if cold was freely applied to the head at the same time. But where copious exudation had occurred, it seemed to be of no advantage. It was very effective in allaying the sympathetic vomiting so often observed early in the disease. M. CHAUFFARD, of Avignon, found it highly beneficial. Dr. W. H. DRAPER gives very decided testimony as to its value, and assuming the disease to be a form of arachnitis, thinks the opium proves good here, as it does in peritonitis, since they are both inflammations of serous surfaces. I have, however, shown conclusively, that it is *not* an inflammation of the arachnoid. The most satisfactory results have been reported by MINER and TULLY, LIDDELL, BOUDIN, HIRSH, FORGET, LEVY, and many others. BROUSSAIS employed it freely after bleeding. These observers, and with them others, deem it the *best* remedy we have, while some, as HIRSH, and, more particularly, MINER and TULLY, consider it almost a specific. NIEMEYER, however, did not obtain such favorable results, though subcutaneous injections of morphia proved beneficial in his experience. The testimony of many others in this country might be added, as well as from Europe, but in the majority of cases they only serve to confirm the good opinion already expressed. The indications for its use, when they are mentioned, or as they appear, agree with those laid down by RUMMEL, from his experience. Here, as elsewhere, in the therapeutical history, the details are left to the judgment and good sense of such persons as this report may chance to reach.

2. *Quinine*.—A few observers have found this remedy beneficial, and especially in paludal districts in this country and Europe. WALES and GERHARD, in a limited experience, found it beneficial. Drs. OTTMAN and BURR, of Carbondale, Pa.,

found it useful in a few cases. But the majority are against its employment. RUMMEL, who seems to have been a very judicious observer, never found it useful, only in the later stages of the disease, when the mind had become clear, and then only as a tonic. Such has been my own experience. MONS. LEONARD and DURAND favor its employment, but evidently in cases where malarial influence has obtained. But HIRSH, whose experience was very extended, and who is a very competent observer, believes it does no good at all. Much the same has been the experience of MONS. GASTE, CHAUFFARD, PASCAL, FORGET, TOURDES, SCHILLIZI, etc. On the other hand, M. GASSAUD lost only 2 cases out of 126, according to his report. This excellent result he attributes to the early and free use of quinia. On the whole, it seems to be useful as a stimulant only in those cases complicated with malarial disease, and at an early period, or as a tonic toward the close of the malady, after the mind is clear, and febrile symptoms have disappeared.

3. *Cantharides*.—This remedy, from its known stimulating effect on the organic nervous system, would seem to promise some benefit. It has not been often tried, but, so far as it has been, the results are favorable. The individual who seems to have had the most extended experience with it, is Prof. ALLEN, of Rush Medical College, of this city. In his experience, and that of some others, it has yielded good results, mostly confined to the early stages, and in cases exhibiting marked depression.

4. *Strychnia*.—Dr. H. NOBLE, in a report on practical medicine, made to the Illinois State Medical Society, details a highly favorable experience with this remedy. Others, as WALES and PALMER, found it quite beneficial. It would seem, its use was attended with the most advantage in the early part of the disease.

5. Camphor, Carbonate Ammonia, Musk, Brandy, Valerian, and the various antispasmodics, have been employed. HALL seemed to obtain benefit from camphor and brandy; GERHARD from brandy; and such has been the experience of several physicians practising in the West in our late epidemics. FORGET

did not observe any advantage to result from the use of camphor, musk, ether, etc.

(b) *External Stimulants*.—External stimulation has proved one of the most efficient means in the treatment of this disease. It has been found useful at all periods of the disease. At the head of the list of external stimulants, stands blistering and counter-irritation along the spine. A few observers have not seemed to derive advantage from their use, but the majority have. DAVIS, of Chicago, NOBLE, THACHER, WALES, ATLEE, of Penn., HIRSH, BROUSSAIS, FORGET, and others, have recorded the most favorable results, as to the employment of blisters from cantharides, at all periods of the disease. RUMMEL highly recommends their use, especially in the middle and later stages of the malady. M. ROLLET obtained excellent results from the actual cautery, freely applied along the spine, especially in low cases. The application of stimulating embrocations and of severe frictions, especially over the spine and on the extremities, and the application of heat, either moist or dry, especially to the extremities, early in the disease, seems to have been attended with marked advantage.

III. *ALTERATIVES*.—They have, generally speaking, not proved useful in the early part of the disease, but have been satisfactorily employed in the latter stages of cases which have some duration, and especially where exudation has occurred abundantly.

1. *Mercurials*.—While some have seemed to derive advantage from them, those who have had the most extended experience have not had favorable results. RUMMEL, HIRSH, FORGET, CHAUFFARD, BROUSSAIS, DRAPER, etc., decidedly oppose the use of mercurials. M. ROLLET would use them prudently. Much the same is stated by M. LEVY. NIEMEYER seems more favorably disposed to their use. They seem to have answered best in complicated cases, and especially those where the secretions are inactive, and in the latter stages of acute cases with exudation.

2. *Iodide and Bromide Potassium*.—The former has proved highly efficient in the latter part of cases where exudation has

occurred. This was especially true in the case of HIRSH and RUMMEL. The bromide of potassium has been occasionally employed with apparent benefit. From its known power of controlling irregular action of the nervous system and its general tranquilizing effect, it would seem to justify a more full trial than it has had. It was tried with benefit in a well-marked case, by Dr. ROGERS, of this city. The permanganate of potash has seemed to be useful, but does not promise much, on account of its unstable chemical constitution.

IV. EMETICS.—I have seen but one author who recommends emetics. There does not seem to be but few, if any, cases in which they are indicated. In the beginning of some cases, it might be desirable to evacuate the stomach, but they are rare.

V. PURGATIVES.—They are often indicated, and have often been employed, sometimes with advantage, at others with no perceptible good, and in some cases apparently with harm. Mild purgatives or laxatives, to unload the alimentary canal, and afterwards to maintain the bowels in a soluble state, seem admissible, if not actually demanded. This, with a judicious use of tonics and a careful diet in the latter stages and during convalescence, constitutes the management found most successful, as I have been able to glean it from numerous observers, times, and places. I have aimed at general results, only in the therapeutical history, not deeming it within the province, as it was not possible, in the limits of this report, to give details of treatment nor specific direction.

By comparing the results of experience in the treatment with the prior therapeutical deductions, it will be found they agree as far as could be expected. This agreement goes to confirm the correctness of the views which led to the deductions just mentioned. I have only space *now* to refer to this agreement, and to say, I have endeavored to approach the examination of this interesting disease without a single prejudgment, and I have also endeavored to conduct my examination at least in full view of the *inductive method*.

I feel that if I had some more time, I might far better elabo-



rate some things in this report, but the limited time in which it has to be rendered, and the difficulty and irregularity of receiving reports from Europe, has made it impossible to do much more.

With these remarks, the report is respectfully submitted.

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ARTICLE XXXIX.

TYPHO-MALARIAL FEVER.

By WM. H. VEATCH, M.D., Pawnee, Sangamon Co., Ill.

Read to the Illinois State Medical Society, June, 1866.

I do not presume that I am the only member of this Society in whose practice has occurred cases of the disease that I am now about to delineate; neither would I arrogate to myself a preëminence in its successful treatment; nor would I say that I desire to present anything to the Society but my own humble experience in the diagnosis and treatment of a disease that, to say the least, has proven itself quite formidable, and one whose diagnostic signs were, in the first cases, so obscure that some time had elapsed, and several cases had been treated by myself and others, before an appropriate appellation could be given it; and even now, some good physicians are disposed to call in question the propriety of applying the name that I think the most appropriate. But as names are applied to diseases, corresponding to their nature and character, and as I believe the profession will sustain me in the name best calculated to convey to the mind of the medical student a correct notion of the nature and character of the disease he is trying to understand, I must be allowed to persist in calling the malady under consideration an epidemic of Typho-Malarial Fever.

A disease of a febrile character, which is manifested by a condition of the human system in which coma is a prominent symptom, we would call typhus or typhoid; and one whose signs manifest all the characteristics of a disease originating from exposure to causes generally known to produce hepatic

forms of disease, we would denominate malarial fever; and a fever that partakes of the nature and character of both the above forms of disease, science would dictate a name corresponding in significance to the nature and character of the double form the disease has assumed.

When a disease makes its appearance in a community and affects those only of a certain district, or is confined in its ravages to a certain class, we call it endemic. But if it prevails generally, we call it epidemic. And so, throughout the whole catalogue of maladies to which flesh is heir, we apply names corresponding to their nature and character.

The term *typho-malarial* carries with it an idea of a double form of disease, just the idea that is desired to be conveyed when speaking of the epidemic now under consideration, for almost all cases of fever occurring under my notice during the autumn of 1864 and throughout the year of 1865, partook more or less of this double character. But without farther discussing the propriety of the name at this point, I will proceed to a partial sketch of its history.

This fever made its appearance epidemically in the vicinity of Pawnee, Sangamon Co., Illinois, during the autumn of 1864, and, as the season advanced, its character was more and more developed, until the beginning of the winter, when it became so malignant that the mortality was somewhat alarming; but towards spring its malignancy abated to some extent. As the warm weather of 1865 set in, its virulence increased, and throughout the whole season was very severe, although the mortality was not so great, owing, perhaps, to the fact that its nature had become better understood, and, consequently, a more judicious application of remedies.

The first cases coming under my care, had all the appearances of a common remittent in its ordinary form of chills—thirst, transient pains through the body, aching of the back and limbs, tongue covered with a white or yellowish coating, sickness of the stomach, restlessness, and a remitting fever—but after from three to seven days of these symptoms, a decided typhus character was assumed, and the patient would

pass rapidly into a condition not unlike the third stage of typhoid fever, but by the time the third stage of typhoid should have made its appearance the patient would either have passed through the apparent third stage of typhoid and convalescence be established, or else he would have sunk into collapse and died, on or before the fifteenth day.

There was one case so peculiar in its progress, that I must delineate its history:—A widowed lady, of 35 years of age, delicate constitution, melancholy disposition, bilious temperament, was taken sick November 26th, 1864, with all the signs of an ordinary bilious remittent, as above described. 27th. A remission, followed by a recurrence of chills, and a more aggravated character of fever. 28th. Remission very slight, no chills, tongue heavily coated and dry, great thirst, extremely restless and manifests a preternatural strength, pulse full and bounding, bowels moving, bilious evacuations, skin pale and covered with a heavy perspiration. 29th. No sleep, notwithstanding the free administration of opiates during the night; pulse 110, full; tongue dry and more brownish; throat shows signs of congestion; soft palate and tonsils have a dark red somewhat puffed appearance. 30th. Countenance anxious, very pale; pulse 120, and small; all other signs as on the day previous. Dec. 1st. Notwithstanding the free use of alcoholic stimulants, a tendency to collapse continued; tongue almost black; pulse 130, very small, and easily compressed; throat shows signs of an exudation of blood. Coma came on by degrees, and she sank on the 2d, with all the signs of collapse of typhus. After death, petechiæ made their appearance over the chest, the abdomen and back being covered with small spots and blotches, from the size of a pea to that of a silver half-dollar, irregularly distributed over those parts, of a dust-brown color, some of them being almost bronzed. No *post mortem* allowed.

Other cases, similar to the above in the beginning, would run on to the eighth or ninth day without any symptoms of a typhus or typhoid character, and, when looking for a convalescence to be fully established, a recurrence of the chills would

take place, followed by an aggravation of all febrile signs, and in a day or two petechiæ make their appearance, delirium set in, congestion take hold of the soft palate and tonsils, heavy perspiration cover the surface of the body, diarrhœa would ensue, collapse and death by the fifteenth day; or, about the fourteenth day, would arouse, as from a profound slumber, and convalescence would be at hand. Other cases would run the course of an ordinary remittent, and, when about to recover, to all appearances, congestion would seize upon some of the vital organs, and soon it would be carried through the whole system and the patient sink under symptoms not unlike those of the pernicious intermittents of the old writers, or, as we usually term it, congestive chill.

All cases under my care recovered if they passed over the fifteenth day, except one, and I think most of the fatal cases proved so on or before the fifteenth day.

A few cases relapsed and died, I have been told, after convalescence had been fully established, but none of that class came under my notice. Yet another class of cases was occasionally met with, not unlike the beginning of the ordinary typhoid of this climate. The patient would feel indisposed for many days before the real attack would come on, indisposition commenced by general lassitude, depraved appetite, and, after some days, headache would come on by degrees, bowels either completely constipated or a watery diarrhœa of some days standing, referable, perhaps, to a dose of pills or other cathartic medicines taken at the beginning of the indisposition, and after from seven to fifteen days of simple indisposition, slight chills, with aching of the back and limbs, would indicate the coming on of a malarious form of disease, but the tongue showing a heavy brown coating down the centre, the tip and edges having a glossy red appearance, and a continued feverish condition of the whole system. After these symptoms had lasted from three to seven days, wild delirium set in, and the patient was said to have typhoid fever. Soon, the throat showed signs of congestion, and often the glands became tumefied and inflamed, and sometimes even suppurate, but more often an ulcer-

ation, not unlike that of diphtheria, would spread over the inner surface of the throat, the alvine evacuations having a muddy water appearance, and in a few hours, or a day or two at farthest, the patient would sink into a profound stupor, which usually terminated in death from seven to fifteen days after the appearance of the first chills; but when stupor did not come on, the patient would pass through a course of typhoid fever, and recover after a course of from fifteen to forty-two days. Not a few cases came under my care, the first symptom of which was the ulceration of the throat, the attendant fever being of a typhus character.

The eruption did not make its appearance at any regular period of the disease, sometimes coming on as early as the sixth or seventh day, and in other cases not until the eighteenth or twentieth, and one case had no signs of the eruption until the twenty-sixth day. In a few cases it could not be seen at all, and in those cases where the eruption was not to be found at any period during the fever, the symptoms were as urgent as in those having the greatest amount of it.

In all cases, the abdominal region presented a field for much anxious thought. I do not remember of treating a case in which there was not more or less trouble with the abdominal viscera; at times, however, it showed no points of interest until after the fifteenth or eighteenth day, when suddenly the abdomen became distended and tender, the countenance anxious and pale, great restlessness, alvine evacuations muddy and more abundant, and without speedy relief must sink into collapse and die.

There is one symptom in this affection that in most cases was a very prominent one, and one that gave me more trouble than any one symptom of the disease, and one, too, calculated to throw the practitioner off his guard unless he is acquainted with it, and, in my opinion, it is one of the best prognostic signs belonging to this form of disease. I have reference to a constant warm perspiration, often beginning with the onset of the disease and continuing throughout its whole course, and those cases in which the perspiration was the most persistent

were also the most obstinate to control, and a greater number proved fatal, in the progress of which the perspiration could not be checked, than in any other class of cases, which gave rise to the expression of the above opinion, that the state of the diaphoresis was one of the very best prognostic signs of the disease. In all cases where the perspiration ran on to a great degree before it could be checked, the bowels became the agent of prostration as soon as the perspiration ceased. It appeared that the work of prostration must be carried on through some channel until the virus of the disease had spent its force in the system and lost its power for harm, or death cut short the debilitating process.

This scourge had all the characteristics of an epidemic form of disease, and in some instances showed signs of contagion, as, frequently, nurses would be subjected to severe attacks; but in all cases of apparent contagion the disease could be accounted for by other existing causes, such as the general epidemic influence to which all were more or less exposed, and the same depressing influences to which those were exposed who had no opportunity whatever to have it communicated to them by contagion, and the great number of such cases would seem to preclude the idea of contagion altogether.

There does not appear to be anything peculiar in the surroundings of this vicinity calculated to lead to a solution of the cause or causes of so malignant a form of disease as this has proven itself to be. The only matter worthy of consideration in this respect is the fact, that all over this county is generated the elements of a koino miasma which is generally admitted to be the predisposing cause of hepatic forms of disease. And, in connection with this, it will be remembered that during the two years of the prevalence of this disease, the earth was completely covered with all kinds of vegetable growths belonging to this climate, the people subsisting to a great extent on them.

The county is in a pretty high state of cultivation, and has a sufficient amount of undulation to prevent, to a great extent, the stagnation of water. Many small streams make their way through the prairies, emptying their waters into the larger

timbered streams, the water being supplied from numberless small springs opening along their banks, constituting circumscribed marshes, from which arises the principal part of the koino miasma. But these marshes do not appear to be sufficiently extensive to have in them the amount of malignancy that this epidemic carried with it; neither do the habits of the people appear to have anything to do in the production of the disease. So far as the generation of an idio-miasma is concerned, the people almost all live in light wooden tenements, and but few of them have cellars.

Then our attention must be directed to some other agent for a full solution of the causes of the affection. I have already said that during the two years of the prevalence of the epidemic we had an abundant crop of all kinds of vegetation, and a great amount of the people's food consisted in vegetable matter. Now, it has been pretty fully demonstrated, that in animals subsisting on vegetable substances a greater amount of carbonic acid is retained in the blood than in those which subsist on animal food; and, according to DALTON, a less proportion of oxygen is retained in the system and a greater amount thrown back by exhalation in the carnivora than in the herbivora, in the proportion of about three to one. We must, therefore conclude that in those persons whose principal subsistence has been vegetable, that the blood is minus the proper amount of life principle, the oxygen, and plus the carbonic acid; consequently the blood is in precisely the same condition of those persons who are being exposed to the idio-miasma of densely populated cities, crowded hospitals, and prisons, where many persons are crowded together, in small, illy-ventilated tenements, which has been considered by the best authors as one of the causes of typhus and typhoid fevers. What chemical changes these miasmata produce in the blood I cannot say; but one important fact is patent to the most casual observer, *viz.*:—That these miasmatic influences exert a very great power of man's functional organization, and the two distinct influences acting on man's vital apparatus at one and the same time must, of necessity, exert a twofold effect in producing disease, and much greater reason



have we to ground this theory when we remember that the action of carbon in the blood produces delirium and, finally, coma, two of the most prominent symptoms in this affection. So, in this view of the matter, we find a solution to the great question of the causes producing the double form of disease, which also puts forever to rest the already exploded sophism, that two distinct causes cannot affect the system at one and the same time. These separate causes not only affect man at one and the same time, but, when each has had its specific effect in the system, they combine in producing an idio-miasma that not only affects the patient himself, but affects, also, those in attendance, thus supporting the popular notion of the contagiousness of the disease.

This epidemic had no respect for age, sex, or condition of life; the infant at the breast, the hoary-headed grandsire, and all the intermediate stages of life were subject to its ravages. But persons from fifteen to thirty years of age appeared to be more subject to it than the extremes of life; and males seemed to be more susceptible to its influences than females, owing, perhaps, to the greater amount of exposure to the causes.

All that is now left to be considered, is the principles of successful treatment. Now, in the treatment of any form of disease, it is proper, first, to consider the cause or causes producing the disease, and, if possible, remove them. But when the causes are so obscure that they cannot be definitely ascertained, we must be guided into a correct course of treatment by closely observing the effect of those causes, and if, happily, we succeed in counteracting them, we are practising from a correct theory, a rational standpoint; and if I should be mistaken as to the specific causes producing the disease, I cannot be mistaken in the effects produced by them. I have just shown that the vital powers of man are vehemently attacked by the venom of the disease; that delirium and coma are prominent symptoms in its course; and that these symptoms probably depend on a carbonized condition of the blood, for every characteristic of the disease proves a general depraved condition of the vital fluid, and, consequently, an inactive condition of all the vital organs.

A treatment, therefore, that will give renewed energy to the wasting vital powers of the system, a treatment that will increase the vital principles in the blood, give energy to the dormant functions of nature's complicated apparatus, arouse the latent powers of absorption and assimilation, bring back a unity of action between the several organs whose functions are so materially disturbed by the action of a blood poison; in a word, a treatment whose object it is to build up trodden down tissue and act in a direct opposite course to that taken by the disease, will, of all others, in my opinion, be the most eminently successfully. If a blood poison produces debility and prostration, it is the first duty of the physician to administer agents that will impart tone and energy. If a blood poison produces a carbonized condition of the blood, it is the duty of the practitioner to have recourse to those remedial agents that will impart oxygen to the vital fluid of life and thereby generate a more energetic electric current through the whole system, that all parts of the vital domain may have increased energy to cast off morbid agents, and that each part may have power to perform the important duties respectively assigned them by nature.

After we have determined upon a rational course to be pursued, another very important question arises:—Out of the whole catalogue of remedial agents at our command, which are to be chosen, and how are they to be employed, that we may best accomplish the end in view? Shall we by purgatives, in the onset of the disease, prepare our patients for stimulants and tonics in the latter stages? I answer, no!

Secondly. Shall we administer emetics and powerful diaphoretics, that we may eliminate poison from the blood, and thus have a rapid reaction of the vital powers to cast off morbid agents? I answer, never!

Thirdly. Must we, by a persistent course of mercury, excite secretion to such an extent that nature, by her own action, may attempt to build up broken down tissue? My answer to this, also, must be emphatically in the negative.

First. By the administration of purgatives in the onset of

the disease, we relax the coating of the stomach and bowels, and cause a pouring out of the fluids of the blood into their cavity, consequently, a more or less severe diarrhœa, characterized by the rice-water or soapsuds evacuations, and, instead of being a benefit, they only prepare those organs to cast off remedial agents in after treatment, by which we desire to build up the strength of the patient in arousing absorption and assimilation.

Second. By the administration of emetics and powerful diaphoretics, we only open up the channels through which the supplies of the great storehouse of nature will be carried off and squandered when the disease has set complete seige to the citadel of life.

Third. Mercury is known to be a powerful defibrinizer of the blood, and, when steadily administered, not only diminishes that property of the vital fluid, but also becomes a glandular irritant, by exciting secretion when there is nothing but the principles of depravity to be secreted. We thus cause an unnecessary routine of action in the secreting organs, and instead of building up the works of defence, we break down our already weakened fortifications. We must, therefore, conclude that the whole character of mercury, as a remedy in this disease, is an unmitigated evil, and if we would save our patients a great degree of prostration, we must give neither emetics, powerful diaphoretics, nor drastic purgatives; and above all, would I warn you against the use of mercury, to excite secretion, until you knew that you have something more than poison to be acted on.

For fear that my position may be misunderstood on this point, I will here say, that I am a great admirer of the above named remedies in their proper and legitimate places, but I must be allowed to say that I do not believe their places exist in the treatment of typho-malarial fever. Our whole plan must be, as I have already indicated, to build up the system by promoting absorption and assimilation, and not arouse secretion too high until we have something in the blood from which to make tissue.

Now that I have pointed out the general principles on which a treatment is to be conducted, in my own humble judgement, I will proceed to the specification of such articles as, in my opinion, will be best calculated to accomplish the end in view.

When we find a patient laboring under obstinate costiveness, we should move the bowels by a mild enema of castile soap and molasses; this will cleanse the lower bowels and allow the contents of the stomach to pass down without irritation, and thus avoid the obstinate diarrhoea so often met with after the administration of calomel, blue mass, colocynth, jalap, or any other drastic purgatives. After the action of the enema, we should direct the patient to have a mild opiate, to promote rest; remove all sources of anxiety from the mind; and allow him to drink freely of a saturated solution of either chlorate or the acetate of potassium. This will oxygenize the blood, cause a renewal of the electric current through the system, prevent the decay of tissue, and also promote absorption and assimilation. After this course has been pursued from twenty-four to forty-eight hours, owing to the condition of the patient, we may add to the treatment a pill composed of chenoidine and ipecacuanha, two grains of the former to one of the latter, made into mass with the oil of black pepper—one to be given every two or three hours; and a Dover's powder at night, where there is not too much delirium, and, where the opiate cannot be used, the extract of hyoscyamus may be combined with the pill with great advantage. This comprises about all the constitutional treatment necessary in ordinary cases. The body of the patient should be washed twice a-day in soapsuds at first, and in the latter stages may be added to the wash alcoholic spirits, to give tone to the integuments of the skin. Applications of sinapisms and fomentations, for local irritations and complications, must not be neglected, particularly when the bowels become distended and tender, and accompanied by diarrhoea, which is almost universally the case at some period of the disease. If diarrhoea set in, which is sure to be the case if purgatives have been freely used in the beginning, astringents are to be used both by the mouth and rectum. If delirium supervene, cold

water or ice to the head. If heavy perspiration comes on, the dilute sulphuric acid must be freely used. Add to this treatment a proper course of nourishment, freely given, and in such manner that the greatest amount can be taken in the smallest compass, such as beef-tea, milk-punch, egg-nog, etc. Close attention to the condition of the bowels is one of the most important matters connected with the whole treatment, and by allowing the bowels to be too freely moved in the beginning, is, in my opinion, the cause of many cases proving fatal that would otherwise recover.

After first emptying the bowels by the use of a proper enema, they should not be moved again during the whole course of the fever; and if they should move spontaneously more than once in three days, astringents are indicated. In this position, I am aware that I will meet with opposition, and that too from some of my medical brethren for whose professional worth I have great respect. But as that opposition is founded on a mistaken notion of the correct theory of successful practice, I must be allowed to sustain the position by a statement of a few successful cases treated by myself, in proof of its correctness.

In my practice, I have demonstrated, to my own satisfaction, that the longer the bowels are held in check, the better for the strength of the patient; the more rapid the convalescence, the less liable to relapse; and from ulceration of the bowels, I consider the patient entirely exempt—a result so often met with where the bowels have been allowed to move too freely in the beginning. Of twenty-two cases treated by myself, those kept the longest without an action on the bowels made the most rapid convalescence; and, conversely, those that were allowed to be moved freely in the beginning of the fever were the most tardy and the hardest cases to control, other circumstances being equal. Of these twenty-two cases, eight were held twelve days; six were held fifteen days; five, sixteen days; and three were held eighteen days. Many others were held from six to eleven days, with the same comparative result.

I do not remember of but one case proving fatal after the bowels being held in check as long as three days, and that case

was so completely prostrated from the effects of carbon in the blood, that, as soon as the bowels checked, the colliquative perspiration took the place of the diarrhœa, as the prostrating agent, and he died six days after the bowels had been moved.

There is, yet, only one prominent symptom, the treatment of which is to be considered, namely, the throat affection. Many were found to have ulceration of the throat among the first symptoms of the disease, and every case in which the persulphate of iron was used as a gargle, the ulceration was almost immediately healed; and in those cases where the throat only took on congestion, the persulphate of iron was the most efficacious of all applications. I have also used with great benefit, the muriated tincture of iron, but as that is so common among all classes of physicians, its special notice here is unnecessary. But as I have never seen the persulphate recommended in this affection, I beg leave to call special attention to it, both as a gargle and as an excellent remedy in case of ulceration of the bowels, in doses of from three to five grains every two hours. This article, in my hands, has proved to be one of the most efficacious I have ever used.

I cannot close this article, without expressing my preference for the use of the gargle over that of the probang, in the throat affection. I have seen many cases in which I am confident that the probang did as much harm as the remedy did good; and in all cases where it is practicable, I would advise the use of the gargle in preference. But in children, or in adults who are so delirious they cannot be made to understand, the probang will have to be resorted to, but then it should be used with care.

In conclusion, allow me to say, I have only attempted to give my own opinions, backed up by my own practice, regardless of authority or the expressions of opinion by other members of the profession, and I think the principles herein set forth can be fully sustained in practice.

## ARTICLE XL.

## EPIDEMIC DISEASES.

By F. B. HALLER, M.D., Vandalia, Ill.

Read to the Illinois State Medical Society, June, 1866.

Upon my return home, about the 1st of April, 1865, I found an epidemic of erysipelas, rubeola, and pertussis, all of which were of a rather mild type, when uncomplicated; but, unfortunately, many of the measles cases were complicated with pneumonia, which rendered them exceedingly difficult to manage. One case that I saw in consultation, proved fatal. In a majority of the cases, the supporting and stimulating treatment had to be resorted to at the very onset of the attack, and in every instance was it required at some particular stage of its progress. Those of an uncomplicated nature, that I was consulted about, were left to good nursing and hygienic means, and made excellent recoveries, much better than those that were unfortunately drenched by their good mothers with the various panacea teas, the most popular of which is the celebrated sheep-tea, which the little fellows had to drink in large quantities, to the shame of their mothers.

There was nothing unusual in the cases of whooping-cough that occurred. In this disease, I used the bromide of ammonia with apparently good effect, and, from my experience with it, am led to believe it an excellent remedy in this disease, when uncomplicated.

The erysipelatous cases presented nothing of interest. They all readily yielded to ordinary treatment with iron and quinia. During the months of April, May, and June, there were a great many cases. About this time, I had several cases requiring surgical operations, and in every single instance, notwithstanding I used every precaution to prevent it, I had traumatic erysipelas to contend with, and came very near losing two of my patients.

Since June, there has been only an occasional case. As these epidemics began to subside, bilious fevers set in, and by



the middle of July, we were into one of the most prevalent epidemics of malarial diseases that has visited this vicinity during the last sixteen years, the time of my sojourn here, and, as usual when we have a good deal of malarious fevers, they were a mild type, and readily yielded to the proper remedies. Ever since then, throughout the entire winter and spring months, have we been having a great many cases of remittent, intermittent, and pernicious fevers, of a much graver character than occurred during the months of summer and autumn. There was this striking peculiarity about these fevers throughout this epidemic, that they required at least one-third larger doses to subdue them than they had at any former period, since I have been practicing in this locality, not only of antiperiodics, but of purgatives, opiates, or any other medicines that might be indicated in particular cases.

Never, during my residence of sixteen years in this place, have I seen these fevers so prevalent in winter and spring months, and the oldest citizens inform me that they never saw the like before. During this epidemic, I have used more mercurials than I ever did before, nor can I call to mind a single instance where a patient was ptyalized, beyond a mere touch of the gums. I am firmly of the opinion that by the free use of this potent remedy I was enabled to relieve my patients much sooner and had them more thoroughly cured, than by any other means I had at my command. Indeed, so happy were the effects of this remedy upon my patients, that for a time I had almost become an old foggy routinest.

During the last summer, we had but very little summer complaint amongst the children, less than ever I saw before, and what few cases there were were easily managed. I do not think we lost a single patient with this disease during the whole season.

During the autumn months, variola made its appearance in a neighborhood about 15 miles south-east of our place, which soon became epidemic, and by the beginning of winter, many of the families in that region had it. Quite a number of these cases were of a malignant character, and only yielded to the

most vigorous treatment. A goodly number of these cases were under the care of my partner, Dr. R. S. HIGGINS, from whom I obtained this information.

In January, it broke out simultaneously in our village and several adjoining neighborhoods, and continued for some time, and, as it was in the first neighborhood, quite a number of the cases were of a malignant nature. In a shanty in the west part of the town, which was poorly ventilated, nearly half those attacked died; and at the poorhouse, amongst the paupers, there were several deaths. What treatment or care these persons received I have no means of knowing.

The other cases were treated, mostly, by my partner, Dr. R. S. HIGGINS, who informs me that when desquamation began, and in some instances even before this stage, he had to resort to the supporting and stimulating treatment, by which method he was exceedingly fortunate, not losing a single case out of the many he treated. The few cases that fell into my hands were of a mild type, and required no treatment but an occasional purgative of castor-oil, and good nursing.

During this epidemic, we have had the very best of evidence in proof of the prophylactic effects of vaccination. There were no sanitary regulations, nor any means used to prevent its spread; nurses, and those who had varioloid in a mild form, mingled promiscuously with the people on the streets and in the avenues of business, and the only thing that was done to guard against its spread was vaccinating and re-vaccinating, which was pretty thoroughly done. Notwithstanding this free mingling of nurses, and those slightly affected, with the populace, it was not communicated in a single instance; and only in a few instances, did those who were exposed as nurses, or otherwise, who had been previously vaccinated, take varioloid. In one family of twelve, all living in one room, is proof most emphatic as to its prophylactic powers; here, an infant died, and the father had it in a malignant form, and not one of those who had been previously vaccinated had it in any form—were perfectly exempt from it, notwithstanding they were constantly in this one room, cooked, eaten, and slept there. There were sev-

eral instances nearly as striking as this. Certainly, these are strong proofs in favor of vaccination and re-vaccination, and it seems to me, all that is necessary to entirely free the human race from this scourge, is to compel every person to be vaccinated and re-vaccinated until it will take no more, and then inoculate with the pure variola virus. By this means, the system would become so thoroughly protected against it, that it would be utterly impossible for them to ever have it, even in the most mild form.

We have had much less pneumonia this winter and spring than usual, but the few cases that have occurred were more difficult to manage than common, many of them resulting fatally. Nearly all the cases that fell under my observation were asthenic in character, requiring beef-tea and stimulants from the beginning of the attack pretty freely. There was, in most of these cases, a marked tendency to congestion of the viscera of the abdomen, and, in a few instances, to spinal meningitis. These cases usually required large doses of quinine, until they were thoroughly under its influence.

Amongst the children that were attacked, during the months of February and March, with lung diseases, there was spinal meningitis in nearly or quite every case that came under my observation. These cases I treated with calomel, tartar emetic, and ipecac until the inflammatory action had subsided, when I put them upon full doses of chlorate of potassa and sulph. of quinine. During the whole period of this disease, up to the establishment of convalescence, I had rubefacients or blisters applied to the spine, with the most salutary effect. All my patients recovered, except one who was poorly cared for.

The past year has been one of great extremes of weather, such as was well calculated to produce more than the ordinary number of cases of sickness. For the last few months we have been having a great deal of easterly winds, more than I recollect to have ever seen before, which I think has been the cause, to a great extent, of the obstinacy of our diseases. Our very old people and those of feeble constitutions have suffered more in portion than the young and vigorous, as compared with other springs.

We have had less typhoid fever this last year, than for many years past, although there were a great many cases of remittent fever that assumed a typhoid character, and such cases were usually protracted and had to be strongly supported. These cases I gave beef-tea and milk *ad libitum*. The result was very satisfactory. Nearly all our cases for the past year have been of an asthenic character. This is becoming more and more so every year.

The lancet is scarcely ever used. Digitalis, tartar emetic, or veratrum viride are only in a few instances called into requisition. Opium will control the inflammatory action in most cases, and with it my patients make more rapid recoveries than under the more active treatment.

This, sir, is about a true character of the diseases that have occurred in our locality during the past year, given in a hasty, desultory manner.

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ARTICLE XLI.

EARLY DIAGNOSIS OF THE BACKWARD CURVATURE OF THE SPINE.

By JULIEN S. SHERMAN, M.D., Chicago.

The importance of diagnosing this disease in its insipieney must be apparent to all. Yet, in a large majority of cases, the difficulty is not suspected until the projection of one or more of the spinous processes reveal the fact, that the disease has already advanced to a change of structure and that a portion of the bodies of the vertebræ have undergone inflammatory softening, followed by absorption, and in many the cavity of an abscess formed.

The early recognition becomes much more important, when the fact is considered, that, the deformity once produced, we can do but little toward restoring the normal form of the spine. It remains a permanent change in its flexibility and symmetry. The arrest of its increase is the object to be accomplished by treatment.

The disease may, properly, be divided into two stages:—

1st. Simple inflammation of the intervertebral cartilages.

2d. Caries, suppuration, and deformity.

Very seldom is the inflammation of tubercular origin, but traumatic, following some injury to the spine, occasioned by a fall or blow upon the back, or violent flexion.

The age of the patient will also assist us in diagnosis; the disease being almost entirely confined to an early age, varying from one to eight years, during which period the spine is yet in the process of development and more liable to injury from slight causes.

Boys are more often the subjects of its attacks than girls, the reason of which is not obvious.

At the commencement of the first stage, the child will be noticed to play less vigorously than has been its custom, its motions are not as free, and there is an appearance of stiffness to the back, and an effort to remove the pressure from the inflamed tissues by resting the elbows on a table or chair, and supporting the shoulders and chest. When reaching to grasp articles from the floor, it will endeavor to accomplish the same object by placing one hand on the knee.

The inflammation, once started and aggravated by motion and pressure, slowly increases. The child now complains of pain, the position of which will aid us in determining what portion of the spine is affected. Seldom, in this stage, is there pain in the back—if present, it is deep seated, and tenderness cannot be detected by manipulation—but it is confined to the stomach and bowels; also radiating on the sides. It is not of a severe or spasmodic kind, but dull and persistent. These symptoms are often attributed to disorders of the digestive apparatus. They will be distinguished by the condition of the tongue and secretions, which, in this stage of spinal disease are not affected. Very rarely, the cervical region is involved, and the pain then is situated in the shoulders and sides of the neck. Difficulty of deglutition and disturbed respiration are sometimes experienced, although no special disease of these organs can be detected. The sleep of the patient is also disturbed, and the pulse increased in frequency.

If, in a period ranging from one to eight months from the commencement of the disease, the back be carefully examined, it will show, in some portion of its extent, a *bulging*; not a sharp *angular* projection, but a rounded prominence. The inflammation is yet limited to the cartilages and has not involved the bones. The projection is, consequently, produced by the approximation of the bodies of several vertebræ, which continues the curve over a larger portion of the spine and gives it the oval appearance.

Soon, the sharp projection of one of the spinous processes of the vertebræ makes its appearance, and the characteristic sharp angle is formed. It has now advanced to the second stage and the deformity has begun. The inflammation has proceeded to the body of the vertebra corresponding to the projecting process; softening and absorption of its structure has taken place, allowing the bodies of the vertebræ above and below to approximate more closely, and give the appearance of a backward dislocation of the diseased one. The destruction is now more rapid, and caries and suppuration soon follow. The membranes of the spinal cord, and sometimes the cord itself, becoming involved, partial paralysis of the spinal muscles often ensues, causing one shoulder to drop, and giving the patient a sidewise motion in walking. If confined to the lumbar region, the same pathological condition causes reflex contractions of the flexor muscles of the thigh, which may be mistaken for hip-disease. The differential diagnosis is, however, easy. There is no pain or tenderness in the hip-joint, and none referred to the knee, so characteristic in that disease.

The angle is sometimes projected laterally, simulating lateral curvature. It may be distinguished from this by its abruptness and sharpness. The lateral form being an even and rounded curve, as it advances it assumes more the backward direction.

The symptoms in the first stage are sufficiently characteristic, that a close observer will not fail to detect the disease. We can seldom err in our diagnosis, when we find them following an injury; and only on their early recognition and prompt treatment can deformity be avoided.

## Book Notices.

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A PRACTICAL TREATISE ON THE PHYSICAL EXPLORATION OF THE CHEST, AND THE DIAGNOSIS OF THE DISEASES OF THE RESPIRATORY ORGANS. By AUSTIN FLINT, M.D., Prof. Prin. and Prac. of Med., in the Bellevue Hospital Med. College, and the Long Island College Hospital; Fellow of New York Medical Academy, etc. Second edition, revised. Philadelphia: HENRY C. LEA. 1866.

This is a well-executed volume, of about 600 pages. The first edition has been long enough before the profession to have its merits well known. The present edition exhibits indications of having been carefully revised and, in some instances, altered in accordance with the more extended opportunities for observation which the author has enjoyed for the last few years in the Bellevue and Long Island Hospitals. The contents of the book are sufficiently indicated by its title; and we freely commend it to the study of the profession.

For sale by W. B. KEEN & Co., 148 Lake Street.

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ELEMENTS OF MEDICAL CHEMISTRY. By B. HOWARD RAND, M.D., Prof. of Chemistry in the Jefferson Medical College. Philadelphia: T. EDWARD ZELL & Co., 17 & 19 South Sixth Street. 1867.

This is a neat little volume of 400 pages, being made up of a syllabus, or full notes, of the course of lectures given by the author in the Jefferson Medical College. The arrangement of the work is as follows:—An Introduction on the Properties and Qualities of Matter, and of Force. Part 1st. On Physics, including Gravitation, Light, Heat, and Electricity. Part 2d. On the Principles of Chemistry. Part 3d. On the Chemistry of the Elements. Part 4th. On Organic Chemistry. And an Appendix, on Weights and Measures; Incompatibility; Antidotes and Precautions in Medico-Legal Examinations; Precautions for Practice; List of Minerals; and Glossary. We think it will be found a most convenient little book of reference, both for students and practitioners.

For sale by W. B. KEEN & Co., 148 Lake Street.

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A MANUAL OF BLOW-PIPE ANALYSIS, AND DETERMINATIVE MINERALOGY.



By WM. ELDERHERST, M.D., Prof. of Chemistry in the Rensselaer Polytechnic Institute. Third edition, revised and greatly enlarged. Philadelphia: T. ELLWOOD ZELL, Pub. 1866.

This is a small volume, and will be found interesting and valuable to all those who wish to study mineralogy and metallurgy.

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### Editorial.

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DEATH OF PROF. D. BRAINARD.—With feelings of deep sadness we record the death of one of the most prominent surgeons in our country. DANIEL BRAINARD, so long at the head of the department of surgery in the North-west, was attacked with epidemic cholera on Tuesday night, and died on Wednesday afternoon, the 10th inst. Although only 53 years of age, a period which should be the vigor of active manhood, it was generally understood that Dr BRAINARD's health had not been good for one or two years past.

At the commencement of his professional career, he took special pains to qualify himself for the practice of surgery, and almost immediately became identified with the establishment of Rush Medical College in this city; the Surgical Chair in which he filled with marked ability until the day before his death. He was never popular as a general practitioner of medicine, and during the last *ten* years of his life his professional practice was restricted almost exclusively to surgical cases. He was not a showy or brilliant operator, but a deliberate, accurate, and successful surgeon. It was in the lecture-room, however, in his capacity as teacher, that he achieved the highest reputation. Possessed of a mind well stored by reading and observation, and thoroughly disciplined by reflection, his didactic instruction was clear, concise, and impressive; giving him an enviable popularity as a lecturer. In his unofficial capacity, as a member of the profession, Dr. BRAINARD was not noted for genial affability, scrupulous regard for ethical rules, or cordial coöperation in the support of medical organizations. He

was not only studious in everything relating to his favorite department, but he often entered boldly upon original investigations and experiments concerning such topics as strongly attracted his attention. Though he wrote no complete work upon any department of medical science, yet the results of his experiments in the treatment of ununited fractures; on the effects of iodine on the poison of serpents; and on the effects of lactate of iron injected into the veins, in the treatment of the cancerous diathesis, will perpetuate his name on all the future annals of our professional literature. Although the strictly professional income of Dr. BRAINARD was never large, yet, by some successful real estate operations, he had secured to himself and his family an ample fortune.

The action taken by the profession of our city, and also by that of Springfield, in honor of the deceased, will be found on the following pages of this number of the EXAMINER.

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THE DEATH OF DANIEL BRAINARD, M.D.—MEETING OF THE MEDICAL PROFESSION.—A meeting of the members of the medical profession was held on the afternoon of Thursday, Oct. 11th, at the Common Council room, to take suitable action in regard to the sudden death of DANIEL BRAINARD, M.D., who was recently stricken down in the maturity of his powers and the zenith of his honors, by the cholera. The meeting was organized by the appointment of Dr. Duck, as Chairman, and Dr. Charles G. Smith, as Secretary.

On motion, the Chair appointed a committee of five to draft resolutions expressive of the regret of the profession at the demise, and evidencing their sorrow and grief at the dread visitation which had thus inopportunistically carried away an ornament to the profession:

The following gentlemen were designated as such committee: Drs. McVicker, Johnson, Trimble, Smith, and Byford, and after a brief absence they presented the following resolutions, which were unanimously adopted:—

*Resolved*, That in the dispensation of Divine Providence, who has removed from our midst our deceased friend and brother,

Daniel Brainard, we recognize the hand of Him who doeth all things well, and we bow submissively to His will.

*Resolved*, That our deceased brother, by his natural powers of mind, by his capacity as a teacher, by his industry, by his untiring zeal and assiduity in the profession of which he was a distinguished member and ornament, has acquired a position of character and usefulness, recognized not only by his colleagues at home, but by the profession throughout the world.

*Resolved*, That, in his example, we read the great lesson of encouragement in the path of duty and honor, and, while humbly seeking to imitate it, we hold it up to the consideration of members of his own and other professions elsewhere, and that in his death the profession of our city and the North-west has suffered an irreparable loss.

After the adoption of the resolutions, Dr. BROCK McVICKAR paid the following feeling tribute to the memory of the deceased:

EULOGY BY DR. BROCK McVICKAR.

The occurrence which has called us together this afternoon, is recited in brief and emphatic words in the daily papers of our city:—Died, in this city, Oct. 10, of cholera, Daniel Brainard, M.D., aged 53 years.

When we were last assembled in this room, gentlemen, it was to record our respect for a departed brother who had been called away in the spring-time of professional life, with the future all bright and promising before him. To-day, we come to pay the same sad office to another, one older in years and full of honors, whose ambition and aspirations of the future had become to him fruition in the present, and who stood, with well-earned laurels, at the head of a profession, which he dignified and crowned.

Daniel Brainard was no ordinary man; he was possessed of high and sterling ability, quickened and developed by culture and study; of untiring industry, of unremitting perseverance and unflagging zeal in the pursuit of knowledge. He was called cold and reticent at times, but what seemed coldness to many was but the absorption of a strong and earnest nature, pushing forward to the attainment of the high mark he had proposed to himself. He had an abiding hatred for pretenders and shams, and looked with little favor upon those who followed their profession without seeking to rise above its daily routine of duty; when once satisfied of a man's integrity, and earnestness, and intelligent devotion to his profession, he was his friend.

With Dr. Brainard, with but a slight interruption, my relations, personal and professional, have always been of the most agreeable character; and, at the time of his death, they were particularly so; and I shall never forget how, during my last interview with him, but a few days since, I was impressed with respect and regard for him, as a gentleman courtly in his manners, polished by foreign travel and culture, the peer of the highest in the land.

His career, gentlemen, may serve to illumine the pathway of the student, to quicken him to higher and better efforts, and give him assurance of success. Of Brainard's success, much feeling has been manifested by smaller minds, many times falling under my own notice. He was restless sometimes under this jealousy and its resulting injustice, and that restlessness toned and tempered his feelings; but when a man has attained the measure of success which fell to his lot, and which he earned so faithfully, he can afford to smile instead of being annoyed, and look down, as he did in his later years, with the calm consciousness of superiority, grafted by his own hand and his own exertions, or powers and faculties given him by God. But he is gone from us, and the places which knew him once shall know him no more again forever. Peaceful be his rest, green be the turf which grows above him, and bright be the place which he shall hold in the memory of the friends he has left behind him.

Drs. Davis, Johnson, and Paoli followed Dr. McVickar, and spoke feelingly and affectionately of the manifold virtues of the deceased, his exalted professional ability, and the endearing reminiscences which would ever follow his memory in their hearts.

Dr. Blaney presented a request from Dr. Powell, the nephew and only surviving relative of deceased in the city, soliciting the meeting to designate six members of the profession to officiate as pall-bearers at the funeral.

The Chair, in consonance with the sense of the meeting, appointed the following gentlemen:—Drs. Hitchcock, Duck, Eldredge, Johnson, Paoli, and Hamill.

Dr. Johnson submitted to the meeting a proposition relative to taking a cast of the features of deceased, previous to interment, and their lasting perpetuation either in statuette or bust models. Several gentlemen expressed their approbation of the

movement, and, in pursuance thereof, a committee, consisting of Drs. Hamill, Johnson, Blaney, Davis, and Miller, was appointed to carry the design into effect.

The meeting then adjourned.

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MEETING OF THE SANGAMON COUNTY MEDICAL SOCIETY.—  
RESOLUTIONS OF RESPECT.—A special meeting of the Sangamon County Medical Society was held in the city of Springfield, October 13th, at 3 P.M. The President being absent, Dr. Wright, of Chatham, Vice-President, took the Chair.

On motion of Dr. Wardner, the Chairman appointed a Committee of three to draft suitable resolutions regarding the death of Dr. Daniel Brainard, of Chicago. The Committee consisted of Drs. Wardner, Griffith, and Bailache.

On motion of Dr. Townsend, Dr. Wright was added to that Committee.

The Committee reported back the following preamble and resolutions, which were unanimously adopted:—

*Whereas*, It has pleased an All-Wise Providence to remove by death, Daniel Brainard, M.D., late President and Professor in Rush Medical College, whose ability and professional attainment gave him high rank in medical science, not only among friends and acquaintances, but among all devotees of the science he so much honored, therefore,

*Resolved*, That in the death of Dr. Brainard, the profession is bereft of an able teacher, science of an ardent student, and the community in which he lived of a valuable citizen.

*Resolved*, That we tender to the faculty of Rush Medical College our condolence for the loss of its President, and one of its most honored teachers.

*Resolved*, That we tender to the afflicted family our sympathies in this, their hour of sad bereavement.

*Resolved*, That the Secretary furnish a copy of these resolutions to the family of the deceased, and the faculty of Rush Medical College. Also, a copy to each of the papers of this place, and to the *Chicago Medical Examiner* and *Chicago Medical Journal*, for publication.

N. WRIGHT, *Chairman*.

A. L. CONVERSE, *Secretary*.

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REPORT OF THE COMMITTEE ON THE SANITARY  
CONDITION OF THE CITY, AND THE PREVA-  
LENCE OF CHOLERA; DURING THE MONTHS  
OF AUGUST, SEPTEMBER, AND OCTOBER, 1866.

By N. S. DAVIS, M.D., of Chicago.

Read before the Chicago Medical Society.

According to previous report, the last three days of July were cool, clear, and pleasant. It remained cool, with a prevalence of north and north-east winds, until the morning of the 8th of August. Showers of rain fell on the 3d, and a steady rain all the night of the 6th and 7th. On the morning of the 8th, the wind had changed to the south; the air was filled with mist or aqueous vapor, and, at midday, was very warm and oppressive. Between 5 and 6 o'clock P.M., the wind suddenly changed to the north, and the atmosphere became so cool as to produce chilliness. It remained cool and clear until the 11th, when the wind changed to the south, the sky became cloudy, and in the evening there fell copious showers, with sharp lightning. The 12th was mostly clear, wind south, and atmosphere exceedingly warm and damp. These conditions continued until 11 o'clock A.M. of the 13th, when the wind again changed to the north, and the air became cool and clear. But at midnight, the wind again changed to the south, and the morning of the 14th was hot, damp, and oppressive, with showers of rain in the afternoon. From this time to the 20th, the atmosphere was still, or moved only slightly by winds from the south or south-east, very damp, and moderately warm. During the 17th and 18th, especially, the air was still and so light that smoke and mist hung close to the earth, instead of rising or of being lifted to the higher regions. Rain fell very moderately in the evening of the 18th, and during the afternoon of the 20th. During the 21st, 22d, and 23d, the wind blew from the north, and the atmosphere was decidedly cold but damp, and steady rain nearly all of the 23d. During the 24th and 25th, the wind continued north, and the air was clear, cold, and dry; so cold, indeed, that an overcoat was required for comfort during the whole day.

From the morning of the 26th to the 31st, the atmosphere was again still, damp, and moderately warm; very similar to that from the 14th to the 20th. It was filled with smoke and mist, especially during the nights. The slight winds that were felt were from the south and south-west, except during the afternoon of the 28th, when it blew from the north-east, and was cooler. Slight rains fell on the 29th and 30th, and in the evening of the 31st, copious showers fell, with thunder and lightning, during which the wind changed to the north-west and blew up a stiff breeze. Vivid flashes of lightning continued until a late hour in the night. It was the first display of the kind during the month.

From the 1st to the 25th of September, the weather was almost continuously cool and rainy, with a prevalence of north and north-east winds. The rains were so frequent and copious that the surface of the earth was kept constantly saturated with fresh fallen water. Occasionally the sun would shine out clear and warm for two or three hours in the middle of the day, but during the whole time named there were not three consecutive warm, dry days. From the 26th of September, however, until the 7th of October, the atmosphere was clear, pleasant, and moderately warm, though the prevalent winds were continued from the north, north-west, and north-east. On the 7th, the wind changed to the south, and became very light. From the 7th to the 13th, the atmosphere was filled with mist and smoke, and so still as to be scarcely moved by a breeze, either day or night. The sky was most of the time clear; and the temperature moderately warm. On the 13th, a light breeze sprung up from the north-east, the air was more bracing, and the fog or mist that had so steadily filled the lower strata of the atmosphere was dissipated in twenty-four hours. From this to the evening of the 19th, the weather was mostly clear and pleasant though warm, and very little wind from any quarter. On the 20th, the wind changed to the south-west and blew strongly, followed by clouds and some rain. The following night, there were copious showers, with thunder and lightning and strong wind from the west. The 22d was cold, nearly clear with a strong west wind, and frost at night. The 23d was cold,



cloudy, with light falls of snow, sufficient to whiten the sidewalks. At this date, the evening of the 24th of October, it remains cold, cloudy, with north-east wind.

During the first seven days of August, cases of typhus fever and dysentery continued numerous, but attacks of cholera infantum and serous diarrhoea were less frequent than in the middle of July. On the 8th, there was, in my practice, a sudden increase of the latter class of affections; and a still more marked increase on the 12th, when I met with one case of cholera at 109 North Water Street, and another on Wabash Avenue, south of Twelfth Street.

On the 6th of August, a company of Mormons left one of their number, a native of Denmark, sick at the railroad depot, from whence he was taken to the County Hospital, located between 18th and 19th Streets, where he died in the evening of the same day with all the symptoms of epidemic cholera. On the 9th, three days after the Dane died, a female nurse and a male inmate of the hospital were attacked. From this time, one or more cases occurred daily among the inmates of the hospital until the 18th, when it ceased. Whole number of cases occurring in the institution, from the 6th to the 18th, was 19; of whom 11 recovered and 8 died. Nearly simultaneous with this outbreak in the County Hospital, cases of cholera occurred in almost every section of the city.

On the morning of the 14th, I was called to three cases widely separated from each other. One was at 63 Michigan Avenue, and another at 323 South Morgan Street. From this time until the 20th, new cases occurred daily in all parts of the city, although the aggregate number did not exceed an average of 20 per day. From the 20th to the 26th, there was a decided decrease in the number of cases, followed, however, by a moderate increase again from the latter date to the end of the month. The whole number of deaths from cholera during the month, as reported at the Health-Office, was 139, or a little more than 4 per day.

From the 1st to the 25th of September, the disease continued pretty nearly a uniform ratio of prevalence, although it was

restricted more closely to the population of foreign birth, and in parts of the city least influenced by drainage and cleanliness. For instance, the whole number of deaths reported from this disease during the month was 166, or an average of 4 per day. Of these, 88 were natives of Germany, 40 of Ireland, and only 20 of the United States. During the last week of September, the disease declined so much that many regarded it as substantially extinct. So true was this, that the editors of the *Chicago Medical Journal* inserted the following paragraph in their issue of the 1st of October:—"In our next issue we intend to give our readers a complete sketch of the course of cholera in Chicago during the present season. The number of cases has been comparatively small, and is rapidly approaching a minimum."

The decline in the prevalence of the disease continued until about the 7th of October, when it began to increase so rapidly that from 18 cases and 7 deaths, reported on the 7th, it reached 66 cases and 17 deaths on the 11th, and 62 cases and 24 deaths on the 12th. This seemed to be the climax of its prevalence, and its decline was almost as rapid as its access. Thus, three days subsequently, namely, on the 15th, the whole number of cases was 38, with 10 deaths; and on the 17th, only 26 cases and 3 deaths. The whole number of cases reported to the Health-Officer, from the 7th to the 21st of October, was 522 of whom 135 only proved fatal. During this sudden exacerbation of the disease, it continued to manifest the same predilection for the population of foreign birth, living in undrained and uncleanly localities, as during the month of September. Thus, of the 522 cases reported, only 130 were represented to be natives of the United States.

If we compare the foregoing figures with the fact that this city contains a population of over 200,000, it will be seen that the prevalence and fatality of cholera during the past season has been far less, comparatively, than during former epidemics.

Although the prevalence of cholera in this city, during the last three months, has been very moderate in comparison with other Western cities, or with former epidemics here, yet, in all

its features, it has been identically the same disease as in 1849 or 1854. The whole number of cases in proportion to the population has been very small, but the severity of such cases as have come under my observation, has been fully equal to those occurring in the former epidemics. I have not been able to trace a single new feature in relation either to the symptoms, pathology, or tendencies of the disease.

*Treatment.*—After three months of daily observation at the bedside of cholera patients, you may expect something from me in regard to the treatment of this dreaded disease. When this subject was under discussion in the Society several months since, I expressed freely my views in regard to the nature of cholera and the details of its treatment; and those views were published in the April number of the CHICAGO MEDICAL EXAMINER. Since then, I have watched, with much care, the effect of different remedies and different modes of treatment. I have seen some cases treated during the active stage with alcoholic exhilarants; others, with opiates; and still others, with salines and evacuants. A young woman, previously healthy, was attacked at breakfast-time, in the morning, with severe symptoms of cholera. A physician reached her in about two hours, and commenced, immediately, the use of brandy. He gave it in moderate, but frequently repeated, doses, until, in from three to four hours, she had taken a full pint of the brandy and some wine. At the end of that time she was in perfect collapse, pulseless, cold, and blue. No other medicine had been given. In four or five hours more she was dead. In no case have I been able to detect the slightest beneficial effect from this class of agents. Those cases, to which I have been called, in which opiates had been exclusively or chiefly relied upon during the active stage, have either had the discharges only temporarily checked, or have exhibited a persistent tendency to stupor, suppression of urine, hiccough, and ultimately death. Similar results have followed a reliance on the various cholera mixtures sold by druggists, consisting of opium, camphor, cayenne pepper, and sometimes rhubarb, in varying proportions. I saw four cases that had

been treated by evacuants. The first was a man who, during the early part of the active stage, drank freely of seidlitz powders. He passed rapidly into a state of collapse and died. The second was a man who said he had taken, at the commencement of cholera symptoms, an emetic, followed by a dose of sulphate of magnesia, and still later by a dose of castor-oil. He did not go into complete collapse, but lingered three or four days, in a state of great exhaustion, and then died, the stomach appearing to have lost all power to absorb even the blandest articles of nourishment. The third and fourth cases had been treated with repeated doses of castor-oil and small doses of brandy. Both terminated fatally. From all my observations the present season, I am forced to the same conclusions in relation to the nature and treatment of the disease as in 1854, and as fully expressed in the former discussion in this Society.

From the 1st of August to the 28th of October, 205 cases of what I regarded as cholera came under my observation. Of these, 102 were in the first stage, or that of simple cholera diarrhoea, of which all but one recovered. When first seen, 77 were in second stage, or that of active vomiting, purging, cramps, etc., of which 18 died, and 59 recovered. When first seen, 26 were in the third stage, or that of complete collapse. By complete collapse, I mean a condition in which the pulse is no longer felt at the wrist; the surface is cold, corrugated, and purple or leaden color; the urine suppressed, etc. Of the 26 seen in this condition, only 2 recovered. One of the two that recovered from collapse was a boy about 7 years old. He lingered in a state of extreme feebleness for ten or twelve days, and presented well-marked ulceration in the lower half of the cornea, in both eyes. His recovery was ultimately complete. The other was a young man residing at 386 Indiana Street.

In making up the foregoing statistics of cholera cases coming under my observation, I have not included any cases of children under two years of age. The prevalence of *cholera infantum* was very severe in July, less in August, and still less in Sep-

tember and October. The treatment on which I have relied in the management of cholera this season, has been substantially the same as I found most efficient in the former epidemics, and, consequently, I need not repeat it here. The question, how far the prevalence of cholera this season has thrown light upon its essential causes, and its relation to certain other diseases, will be considered more fully in another communication to this Society.

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EASILY FRIGHTENED.—It seems that when the cholera began rather suddenly to increase in this city during the 8th and 9th of October, a part of the Faculty and students in Rush Medical College became decidedly alarmed; and on the morning of the 10th, at the suggestion of members of the Faculty, a large majority of the students voted to have the lectures suspended, that they might immediately leave the city. A suspension, consequently, actually took place, and the lectures were not resumed until the 22d of the same month. When it is remembered that the highest number of deaths from cholera in any one day during the present season was 24, in a population of 200,000, it must be confessed that our neighbors in Rush Medical College were rather easily frightened.

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POISONOUS PRINCIPLE OF MUSHROOMS.—Dr. Letelleir says, that the poisonous substance in mushrooms is a fixed, non-crystallizable, narcotic principle—amanitine. It is precipitated by iodine and tannin. The treatment for poisoning thence resulting is vomiting and purging, followed by a strong aqueous solution of tannin.—*British Medical Journal*.

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CRYPTOGAMOUS ORIGIN OF INTERMITTENT FEVERS.—The editors of the *Atlanta Medical Journal* state that a friend, led by the experiments of Dr. Salisbury, is investigating this subject, and will probably communicate something valuable in regard to it.

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OBITUARY RECORD.—Died, in Boston, Sept. 15th, 1866, of cholera, after an illness of only a few hours, Augustus A. Gould, M.D., a very eminent physician and accomplished naturalist, and late President of the Massachusetts Medical Society.

## MORTALITY FOR THE MONTH OF SEPTEMBER.—

## CAUSES OF DEATH.

Accidents, .....	14	Fever, Scarlet, .....	12
Apoplexy, .....	2	Fever, not stated, .....	4
Burned, .....	1	Flux, .....	1
Cancer, .....	4	Hydrocephelus, .....	3
Childbed, .....	5	Inflammation of Kidneys, .....	1
Cholera, .....	166	Inflammation of Brain, .....	4
Cholera-Morbus, .....	36	Inflammation of Bowels, .....	14
Cholera Infantum, .....	6	Inflammation of Lungs, .....	7
Colic, .....	1	Inflammation of Stomach, .....	1
Congestion of Brain, .....	2	Killed, .....	3
Consumption, .....	29	Measles, .....	14
Convulsions, .....	20	Marasmus, .....	1
Croup, .....	6	Old Age, .....	13
Chicken-Pox, .....	1	Phthisis Pulmonalis, .....	1
Disease of Brain, .....	1	Pneumonia, .....	2
Disease of Heart, .....	11	Scrofula, .....	2
Disease of Liver, .....	1	Small-Pox, .....	1
Disease of Kidneys, .....	1	Stillborn, .....	6
Delirium Tremens, .....	4	Summer Complaint, .....	87
Decline, .....	1	Suicide, .....	3
Dropsy, .....	11	Teething, .....	26
Diarrhœa, .....	23	Tuberculosis, .....	1
Diphtheria, .....	9	Whooping-Cough, .....	40
Dysentery, .....	13	Worms, .....	1
Drowned, .....	7	Wound, .....	2
Epilepsy, .....	2	Unknown, .....	53
Fever, Typhoid, .....	35		
Fever, Childbed, .....	2	Total, .....	739
Fever, Remittent, .....	16		

AGES OF THE DECEASED.—Under 5 years, 329; over 5 and under 10 years, 48; over 10 and under 20, 36; over 20 and under 30, 77; over 30 and under 40, 106; over 40 and under 50, 68; over 50 and under 60, 40; over 60 and under 70, 22; over 70 and under 80, 3; over 80 and under 90, 3; unknown, 7. Total, 739.

## NATIVITIES.

Chicago, .....	311	Ireland, .....	83	Russia, .....	1
Other States, .....	108	Norway, .....	25	Switzerland, .....	1
Canada, .....	3	Bohemia, .....	17	Wales, .....	1
Denmark, .....	2	Scotland, .....	4	Sweden, .....	15
England, .....	15	Finland, .....	2	Unknown, .....	8
Germany, .....	122	Italy, .....	1		
France, .....	4	New Brunswick, .....	1	Total, .....	739
Holland, .....	6	Poland, .....	3		

**CHOLERA HOSPITALS.**—The following is the substance of a report drawn up by the Council of the Epidemiological Society, and is of unusual interest at the present time:—

I. There appears to be a general concurrence of opinion, expressed or implied, that, under certain circumstances and conditions, cholera is liable to be communicated from person to person; the liability being usually in proportion to the crowd-

ing of many persons together, the defective ventilation of apartments, and the neglect of thorough cleanliness in respect of person or abode.

In addition to the possible risk of the extension of the disease from this source, the alarming character of the symptoms, and the necessity for unremitting attendance upon the sufferers, are calculated to produce terror in the minds of spectators, and thus strongly predispose them to be attacked during an epidemic season.

For these reasons, the opinion is very generally held that it is unadvisable that cholera patients should be admitted into wards which are occupied by other sick inmates.

The experience, however, of some of the metropolitan hospitals in past epidemics, shows that, due attention being paid to sanitary arrangements, cholera patients may be received, in limited numbers, into the general wards without injurious results either in the other sick or to the ordinary attendants.

No instances have been referred to, in the evidence before the Council, in the opposite direction, *viz.*:—of the disease having spread to the other inmates of a ward in a well-regulated hospital.

II. With respect to the second query, the experience of the metropolitan physicians who have favored the Council with replies, appear to be that, with proper precautions, cholera patients may be admitted into separate wards in general hospitals or infirmaries without undue risk of the extension of the malady to the other inmates of the institution.

This opinion is shared by all the respondents who have had experience of the disease in tropical countries.

It would have been very desirable to have been informed of the results on this point, in some of the military and naval hospitals in this country and also abroad.

The precautions above referred to are these:—(a) Ample space to each patient; not less than 1500 or 2000 cubic feet. (b) Thorough ventilation of the wards at all times, both night and day. (c) Immediate disinfection and removal of the excreta, soiled linen, etc. (d) A separate staff of nurses.

III. The reply to the third query depends much on the opinion formed in respect of the two former questions. If cholera patients are not admissible into general hospitals or infirmaries under any conditions, it is obvious that some extemporized and special arrangements must be provided for the reception of the destitute when attacked.

But even when they are admitted, there are various circum-



stances in which it will be advisable or necessary that special hospitals should be provided, *e.g.* (a) When general hospitals or infirmaries are at a distance from the seat of the actual or apprehended outbreak. (b) When there is a want of accommodation, with due regard to the ordinary patients, or when the accommodation is unsuitable or objectionable.

In selecting the site of special hospitals, the following points require to be attended to:—(a) Nearness, if possible, to the chief seat or seats of the outbreak. It is important that cholera patients should not have to be carried far. There is, moreover, great risk in moving patients in, or verging to, the state of collapse (b) Airiness, and freedom from intrinsic or contiguous sources of atmospheric pollution. (c) A dry soil and raised situations are, of course, always to be preferred to a low and damp one.

Amid the crowded districts of a large town, it appears preferable that several small and suitable hospitals, or “houses of recovery,” should, if possible, be established in different localities, rather than one or two large hospitals for the reception of a large number of cholera patients.

The remark that the presence of an experienced staff of medical officers in general hospitals, and the existence of more complete appliances of every sort in them than is likely to be provided in extemporized special hospitals for the treatment of cholera patients, are marked advantages in favor of the former, deserve consideration.

The general conclusions of the Council are these:—

1. That it is, on the whole, unadvisable that cholera patients be admitted into the ordinary wards of general hospitals or infirmaries.

2. That cholera patients can be safely admitted into special wards in general hospitals, due precautions being taken; and therefore, that it is desirable, as an important means of providing accommodation for the destitute when attacked, that the authorities of these institutions grant this valuable benefit to the public.

3. That it will be often necessary that special hospitals be provided in aid or in lieu of general hospitals and infirmaries.

In addition to these arrangements for the accommodation of the poor when attacked with cholera, the Council would recommend that places of refuge be provided for the temporary sojourn of some of the unattacked inmates of unwholesome dwellings and localities where the disease has appeared.—*Lancet*, July 28, 1866.

VALUE OF SEWAGE.—The Reclamation Company, now actively engaged on the north side of the Thames, has already tested the value of the metropolitan sewage, and contemplates its regular and systematic utilization on a farm which the company is about to purchase.

We give the following details, as the result of its first labors, which, whether viewed in a sanitary or commercial light, we take to be of the highest importance. Early in April last, a plot of waste land at Barking Creek, devoid of surface soil, was covered with common sand, brought from Mappling, near Shoe-buryness, to the extent of two feet thick, on which was sown grass seed. The surface was then well irrigated with ordinary London sewage from the northern outfall. This has been repeated once or twice. The effects of this *manufacture of green meat*—for such, indeed, it may be most justly considered—has been the production of *three crops*; one cut in June and July, at the rate of sixteen tons per acre; a second, of eight tons; and a third now growing, and almost ready for the scythe! This illustration of the value of what we have for generations cast into our rivers as waste, almost surpasses belief, and, at any rate, ought to engage the serious attention, as it is now doing, of all practical economists.—*Lancet*, Sept. 8, 1866.

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Oct. 1866. 1y.

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R. J. PATTERSON, M.D., Professor of Medical Jurisprudence.

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